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Anonymous Online Survey Detection (and Journalist Verification) of Political Violence, Social Unrest, and Human Rights Violations: Results from Bangladesh and Pakistan

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Abstract

Multiple large bodies of scholarship engage with questions directly concerned with political violence, social unrest, and human rights abuses. Yet, efforts to collect data on these variables are fraught with challenges, and many extant empirical findings rely on data (particularly news report based events) suspected of or known to be biased in aggregate. We explore the use of anonymous, online surveying to detect otherwise unobserved activity. We run anonymous, online surveys in Bangladesh and Pakistan in the run up to, during and in the period following recent contentious 2024 elections in both countries and, separately, in the immediate aftermath of Bangladesh's 2024 Student–People's Uprising and expulsion of then-Prime Minister Sheikh Hasina. To assess the efficacy of the surveys, we partnered with professional journalists working on both countries to verify the authenticity of reported incidents. Results confirm their effectiveness in uncovering many instances of political violence, social unrest, and human rights abuses otherwise likely to be missed or excluded from major news media reporting and ultimately major datasets derived from it. Yet, they also suggest that anonymous online survey responses and leading event datasets effectively complement, rather than substitute for, one another. Such surveys can be deployed rapidly to communicate with some of the most difficult to reach populations globally about the most sensitive political issues of interest to social scientists and policy professionals.

Keywords: Survey, Internet, Political Violence, Human Rights, Unrest, Journalism, Data Missingness

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1. Introduction

Political violence, social unrest, and human rights abuse are central to numerous bodies of research.[1] This includes their causes (De Mesquita, 2010; Fearon and Laitin, 2003), consequences and aftermath (Bauer et al., 2016; Daly, 2021; Davenport et al., 2019; García-Ponce et al., 2023; Ghobarah et al., 2003; Grossman et al., 2015; Narang, 2014), and internal dynamics (Christia, 2012; Esberg, 2021; Magaloni et al., 2020; Milliff, 2024; Dube and Vargas, 2013; Rigterink, 2021; Schnakenberg and Wayne, 2024; Sexton, 2016), how these phenomena interact (Balcells and Solomon, 2020; Van Baalen, 2024), and their prediction (Bell et al., 2013; Blair et al., 2017; Hirose et al., 2017) and evolution (Cingranelli and Filippov, 2018; Fariss, 2019).

Yet, theorizing and the development of causal identification methods have greatly outpaced progress on fundamental measurement of variables capturing these phenomena. Careful measurement is fraught with challenges. Much as Lyall et al. (2015, p. 833) observed about human intelligence during conflict—"counterinsurgents seek it, insurgents safeguard it, and civilians often trade it"—constraints on educing fundamental details of political violence, social unrest, and human rights abuse are similarly severe: journalists selectively report them (if able to access affected regions); political actors strategically suppress, distort, and misreport them; and civilians face retaliation for sharing them. Existing work finds that particular incidents of political violence and social unrest are systematically underreported by news media report based datasets—the backbone of micro-empirical research. Data limitations on human rights abuses are more severe: leading datasets track broad, year-to-year changes across countries, effectively preventing microempirical research in the first place.
In this article, we test whether anonymous, online surveys are effective in identifying incidents of political violence, social unrest, and human rights abuse (hereafter, violence, unrest, and abuse for succinctness), potentially helping circumvent severe informational barriers to empirical conflict and human rights research. To do so, we ran surveys in Bangladesh and Pakistan using random domain intercept—a survey technique that involves inviting potential respondents who navigate in error to dormant websites (visiting the domain of a business that had expired, following erroneous key stroke, et cetera). We asked participants whether they had recently observed, or otherwise had direct knowledge of, various incidents related to these categories. We timed our surveys to overlap with the contentious 2024 elections occurring in both countries. A second survey in Bangladesh captured the immediate and continued aftermath of the 2024 July Revolution, which led to the expulsion of then-Prime Minister Sheikh Hasina. Critically, our surveys took place around major events not just during them. Shaver et al. (2021)

[1] For reviews, see Balcells and Stanton (2021); Berman and Matanock (2015); Berman et al. (2018); Chenoweth (2013, 2023); Clarke (2025); Garíia-Ponce (2025); Goemans and Carter (2025); Kalyvas and Naghizadeh (2025); McCarthy (2014); Nordås and Cohen (2021); Valentino (2014); Wilkinson (2009).

find that news media reporting on crises often drops off well before they have concluded, directly impacting individuals' (lack of) knowledge and (mis)perceptions of issues. By running surveys both before and after the contentious elections as well as during the post-revolution period, we explore what stories these potentially significant but less focused on periods tell in terms of ongoing violence, unrest, and abuse. Ultimately, 36,516 individuals answered at least some survey questions, reporting 15,227 incidents.

In parallel, working with local journalists with experience reporting for some of the world's largest news agencies, we assessed the responses by verifying a large, randomly selected subset. We compared *validated* incidents against a leading conflict event dataset to determine whether anonymous, online surveys not only effectively identify such activity (they do) but whether the activities they uncover are otherwise likely to go undetected (they are).

First, we show that anonymous, online surveys can be used to effectively track ongoing violence, unrest, and abuse, offering a safe (for respondents and those collecting information) and relatively inexpensive means of collecting information of significant interest to scholars and policy professionals. The approach allows scholars to communicate with otherwise out-of-reach populations in near-real time, offering significant advantages over existing collection methods. (e.g., we later describe the collection of civilian safety perceptions across Afghanistan during the days and hours of the Taliban's rapid re-seizure of the country.)

Second, we find that the method uncovers incidents *systematically* underreported by the news media, elucidating the nature of missingness within major event datasets by identifying specific selection dynamics. Furthermore, by comparing verified survey reported events with a separate incident set captured in parallel by the journalists, we find that anonymous online surveys are especially effective in unearthing activity otherwise likely to elude collection efforts by circumventing both *editorial* and *capability* biases inherent in leading event datasets. For dataset curators, these findings provide a road map for targeting future collection efforts, potentially introducing additional data streams to augment media-based curation. For data users, they provide direction related to where or how statistical adjustments to media-based data might be made. Finally, analyzing differences in reported events by types, we find that surveys and a leading conflict event dataset show distinct, complementary patterns in their detections: for some types, the surveys uncover much more activity, while leading event data are superior for others. Rather than serving as substitutes, the data sources combined likely provide a closer approximation of the universe of cases than either alone.

Third, the research offers a proof of concept for tracking human rights violations at the incident (rather than country or regime-year) level,[2] potentially paving the way for the expansion of microempirical human rights studies (Cordell et al., 2022). The incident-level data we collected paint important, distinct stories about the nature of human rights abuses relative to leading annual human rights reports. Coupled with incident-level conflict and social unrest data, they also allow for integrated focus on political violence and social unrest as both causes and consequences of human rights abuses (Cingranelli et al., 2019): "human rights violations are often the root causes of conflict and insecurity which, in turn, invariably result in further violations of human rights" (United Nations OHCHR).

Fourth, potential policy implications are significant. Various governmental or intergovernmental actors rely on reports of violence, unrest, and/or abuse. For instance, international criminal prosecutors—often unable to directly conduct investigations within active war zones—depend on basic details of possible war crimes committed from news reports and other secondhand sources. Similarly, in producing various reports (from the U.S. State Department's annual human rights reports to outputs of the U.S. Commission on International Freedom), such entities are often limited by the information to which they have access from news reports, et cetera. Yet it is not simply lack of (news media, NGO) reporting on individual incidents, but also lack of detail when incidents are reported that can limit their values as key details.

[2] Cingranelli and Richards (2010); Gibney and Dalton (1996); Gibney and Barnes (2023); Rummel (1994).

2. Data Collection in Violent, Oppressive, or Otherwise Limiting Settings

Identifying and collecting details about incidents of violence, unrest, and abuse in conflict, fragile, repressive or other such environments is beset by challenges. In-person surveying can pose risks to those supplying information, "incur[ring] threats by state and nonstate actors, stigmatization, and social ostracism" (Isaqzadeh et al., 2020, p. 174). During active conflict, consequences can be more severe, to say nothing of the (logistical, financial, and safety) challenges faced by those carrying out such work. Safeguarding personally identifiable information can prove challenging as when "data gathered in the countryside [is] brought to the capital, often through military [and occasionally] insurgent checkpoints" (Wood, 2006, p. 381). In rare instances, governmental or intergovernmental entities collect and release high-quality administrative data; though, such cases are the exception to the rule.[3] In practice, such releases often occur long after the fighting has stopped, limiting their use.[4,5]

2.1 The Use of News Media Reports in Conflict or Social Unrest Event Datasets

Accordingly, efforts to track or catalogue incidents of violence and unrest on an ongoing, multicountry, timely basis generally rely on press reports, the primary or sole input to major conflict event datasets, and ultimately the basis for much microempirical scholarship (Raleigh et al., 2010; Leetaru and Schrodt, 2013; Sundberg and Melander, 2013; LaFree and Dugan, 2007; Boschee et al., 2015; Salehyan et al., 2012).[6] Unfortunately, news report-based data systematically underreport political violence (Croicu and Eck, 2022; Zhukov and Baum; Shaver et al., 2022) and social unrest (Clarke, 2023). Patterns of missingness occur geographically— both within countries (Weidmann, 2015; Dietrich and Eck, 2020; Eck, 2012; Kalyvas, 2004) and across them (Behlendorf et al., 2016)—and temporally (Von Borzyskowski and Wahman, 2021).

[3] Notable cases include data releases related to the U.S.-led wars in Afghanistan (Condra et al., 2018), Iraq (Berman et al., 2011; Shaver and Bollfrass, 2022), Iraq and Syria during the U.S. campaign against ISIS (Shaver et al., 2022), and the Philippines (Crost et al., 2016).

[4] Administrative records may reflect their own underreporting biases (Gibilisco and Steinberg, 2023).

[5] While we focus on event-by-event detection, we note efforts by scholars to develop alternative measures like conflict diffusion (Kikuta, 2022; Schutte and Weidmann, 2011).

[6] We note important emerging alternative and supplemental data collection efforts including those facilitated by language models aiding the identification of incidents of violence or unrest from alternative sources (Hu et al., 2022).

2.2 Lack of Event-Level Human Rights Data

For human rights abuses, data limitations are more severe. Data is generally unavailable at the incident level: "counting repressive events is difficult because state leaders have an incentive to conceal actions of their subordinates and destroy evidence of abuse" (Fariss et al., 2020) Tracking violations by substate actors is often similarly restricted—e.g. journalists may lack the resources to safely navigate areas under rebel control or disputed by them.

Most existing measures are, therefore, inherently broad, tracking year-to-year cross-country changes. This includes major human rights datasets at the core of empirical human rights work, CIRI Human Rights Data Project (Cingranelli and Richards, 2010), the Political Terrorism Scale (PTS) (Gibney and Dalton, 1996; Gibney and Barnes, 2023)—which "dominate the statistical study of human rights" (Fariss and Dancy, 2017, p. 274)—the Human Rights Measurement Initiative (HRMI) (Brook et al., 2023), and various, more focused datasets.[7] ACLED (Raleigh et al., 2010) is a notable exception as it tracks various events related to human rights abuses, and our comparisons will extend to those. Though, its inclusion of abuses is not systematic across time and countries. Consequently, such country-level data, "while useful for cross-national comparisons, are likely to be far less useful for internal analysis and completely inappropriate for understanding the lived experience of those suffering from human rights abuse" (Clay, 2016).[8,9]

Furthermore, scholars have raised various concerns relating to the primary reports (annual human rights reports separately produced by the U.S. State Department, Amnesty International, and Human Rights Watch) upon which leading human rights datasets are based. Human rights reporting is "deeply politicized," with the effect that patterns of reporting "do not reflect the impartial application of central values and agreements … but rather the political interests of individual states" (Terman and Byun, 2022). Foreign lobbying (Pevehouse and Vabulas, 2019) and U.S. alliances (Qian and Yanagizawa-Drott, 2017) are found to be responsible for variation in U.S. human rights reporting by countries—with direct consequences for resulting measures derived from such reporting (Nieman and Ring, 2015). Scholars also point to the general lack of transparency regarding data collection and reporting processes.

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[7] Akbaba et al (2011); Walsh, James et al. (2023); Conrad et al. (2013); Databanks International (2023); Banks and Wilson (2021); Harff (2003); Rummel (1994); Harff and Gurr (1988).

[8] Clay (2016) was referring specifically to CIRI; though, the point applies more broadly.

[9] We note important exceptions to broad country/regime-year data. However, these typically cover one or few countries and/or are also based primary on news media reports—e.g., Camera-Recorded Extrajudicial Executions by the Islamic State (2015-2020) dataset (Tinnes, Judith, 2022); Lynchings in Latin America (LYLA) dataset (Nussio, Enzo and Clayton, Govinda, 2022).

The “reports, policy briefs, and other public presentations”—documents upon which leading datasets on human rights abuse are based—“commonly fail to adequately report specific information regarding the research methodologies used” (Rothenberg, 2019). Thus, even basic patterns such as whether trends in the number of human rights abuses over time remain contested amongst scholars in this field (Fariss, 2014; Cingranelli and Filippov, 2018).

2.3 The Potential Promise of Anonymous, Online Surveying

Anonymous, online surveys may facilitate identification of otherwise difficult to detect incidents of violence, unrest, and abuse of interest to social scientists, policy professionals, and journalists. Our use of online surveys, with their emphasis on respondent anonymity, addresses the common methodological challenge of eliciting accurate, uncoerced responses in conflict settings that has been the focus of much social science inquiry and experimentation (Bullock et al., 2011; Blair et al., 2015, 2014; Blair and Imai, 2012; Rosenfeld et al., 2016). Importantly, by allowing for direct questions, the approach that we test in this project avoids many of the logistical and methodological constraints associated with the list, endorsement and randomized response approaches. However, for scholars particularly concerned about lingering bias, these approaches may be integrated into anonymous online surveys.

We describe their substantial promises. First, properly implemented, anonymous, online surveys effectively eliminate threats to respondents otherwise associated with in-person (or other less secure) collection methods.[10] Organizations like RIWI, which undertake such sensitive survey work, employ various technologies and strategies to maximize respondent anonymity, avoiding the risks of in-person surveying or interviewing.[11]

Second, they allow for near real-time event detection. The associated benefits are substantial— from generating empirical insights associated with events as they are unfolding, potentially establishing direct causal effects between variables in a manner that is much more difficult to detect ex post*,* to avoiding the potentially degrading effects of (lack of or partial) memory on response quality when surveys are administered long after the fact (Strube, 1987).

[10] There are also various drawbacks and limitations to this approach, as discussed in Section 6.

[11] For example, interview notes or laptops containing such information may be confiscated; individuals may be seen talking to interviewees (Ellinas, 2021; Curini and Franzese, 2020)

Third, they circumvent numerous constraints that hamper, if not prevent, alternative collection methods. From safety to financial to logistical to administrative barriers, many factors prohibit journalists, survey teams, and others from detecting relevant issues. For instance, the surveys can reach individuals within communities otherwise inaccessible given government or rebel travel restrictions and check points. They overcome urban reporting biases previously identified (Kalyvas (2004)); so long as respondents have access to the Internet (a point to which we return), such surveys can reach rural respondents as easily as their urban counterparts.

Fourth, a major limitation of existing media-based data are inherent editorial biases. Many events are known to the media but not reported given editorial preferences for, e.g., novel, large-scale, and deadly events. Anonymous, online surveys need make no such distinctions.

Taken together, anonymous, online surveys can be used to bypass various *capability* and *editorial* biases (adopting the theoretical constructions independently offered by Parkinson (2024); Shaver et al. (2022)) that plague current data. Indeed, existing results generated from such surveys offer compelling—albeit preliminary—evidence of their ability to surface incidents of violence, unrest, and abuse otherwise likely to remain undetected.

For instance, consider attempts to study unrest within the Lake Chad Basin region, a region to which travel is complicated by potential exposure to "crime, terrorism, civil unrest, kidnapping, and armed gangs."[12] Comparing anonymous survey reports of protest activity in Cameroon, Chad, Niger, and Nigeria collected by RIWI against those captured by ACLED, Shaver et al. (2022) find that for a substantial proportion of administrative unit-month observations, news media-based data showed no social unrest; in contrast, those same units frequently showed survey reported protest activity. Indeed, in the most extreme case of Niger, nearly three quarters of such observations involved survey reports of protest while ACLED showed none.

These results are particularly striking given very low Internet penetration rates across the region (Chad and Niger, in particular, at 18 percent and 22 percent, respectively).[13] Separately, Tandon and Vishwanath (2022), studying of Yemenis' attitudes toward violence, found this population was more likely to share sensitive information through an anonymous, online survey than a mobile phone survey, suggesting that the former may hold advantages to ICT-based alternatives.

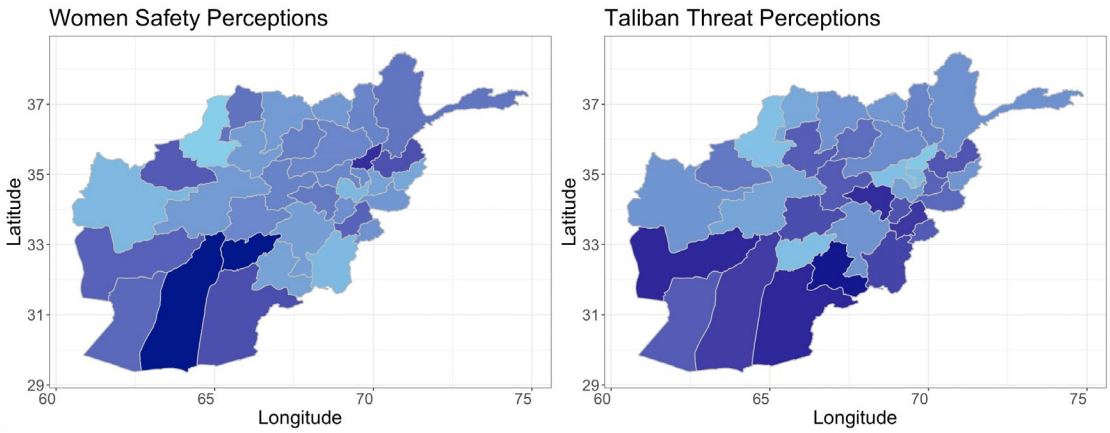
[12] See the U.S. State Department's travel warnings for Cameroon, Chad, Niger, and Nigeria.

[13] The figures for Cameroon and Nigeria are 46 percent and 55 percent, respectively (ITU, 2022).

2.4 The Fall of Afghanistan, Abductions Within the Lake Chad Basin Region, and Protests in Iran

To motivate the primary tests of this paper, we build on the preliminary findings reported above, analyzing responses collected using anonymous online surveys in three major cases of violence and unrest. First, we analyze RIWI survey data collected in Afghanistan during the critical period covering the United States' rapid withdrawal from the country and the Taliban's concomitant power seizure.[14] As the Taliban regained control of the country, Afghans were asked whether they perceived their lives to be at risk from the group and if women were able to work safely where the respondents lived. Plots of their responses (Figure 1) depict clear, significant geographic variation as the takeover occurred, demonstrating anonymous, online surveying's value in rapidly detecting outcomes of interest within highly localized areas—with implications for crisis detection and response (Mueller and Rauh, 2018).

Figure 1. This figure displays province-level survey reported perceptions of women's ability to work safely (left) and the threat posed by the Taliban (right). (Darker shading represents greater perceived threats.) Responses were collected during and immediately following the United States' withdrawal from Afghanistan and as the Taliban seized control.

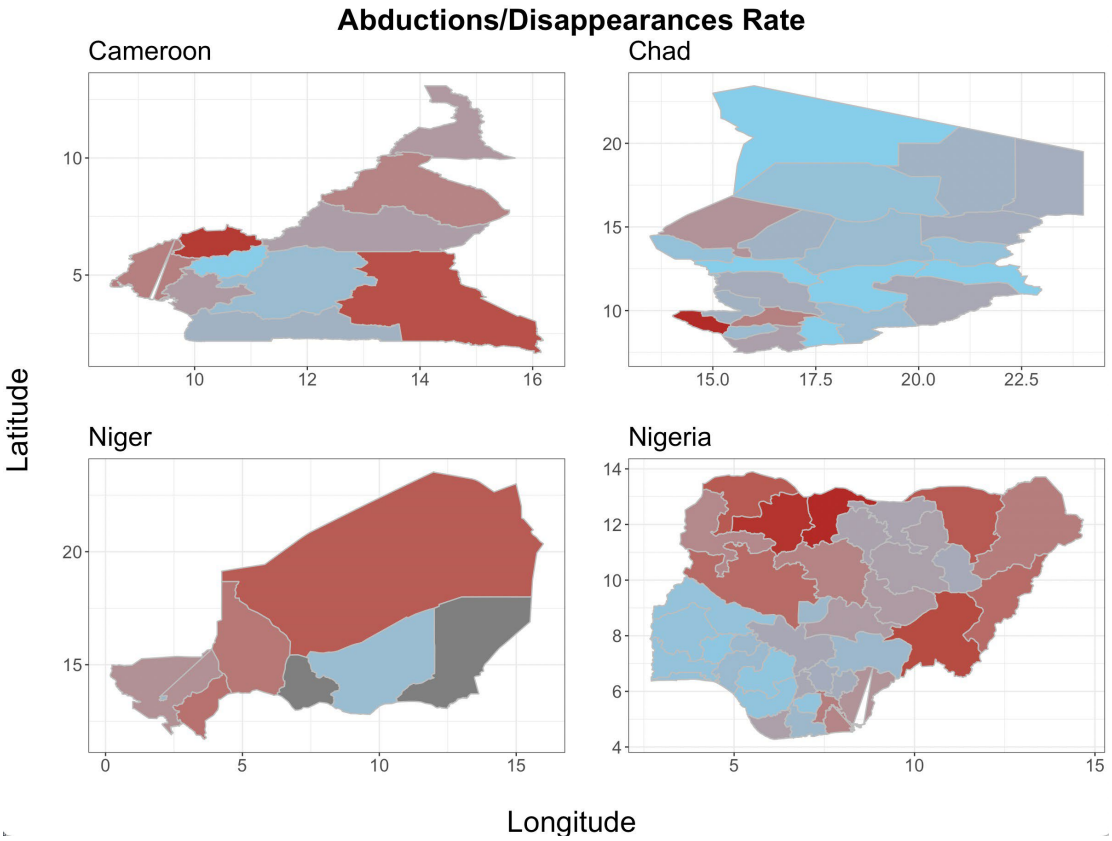


Next, using RIWI survey data collected across Cameroon, Chad, Niger, and Nigeria, we explore spatial patterns of reported abductions and forced disappearances during most months of 2020. This region of Africa is long plagued by insurgent, criminal, and other violence perpetrated by groups like Boko Haram and the Islamic State–West Africa Province, killing some 350,000 individuals in Nigeria alone as of 2020's end (Reuters, 2021) and displacing hundreds of thousands in the region as refugees and asylum seekers over just the past several years (UNHCR, 2024).

[14] August 27–November 1, 2021.

We calculate rates of reported abductions and forced disappearances that vary widely across the countries (Figure 2), substantiating the importance of (and need for) systematic data collection facilitating the sort of microempirical research now prevalent within substate conflict studies but rare within the human rights literature.

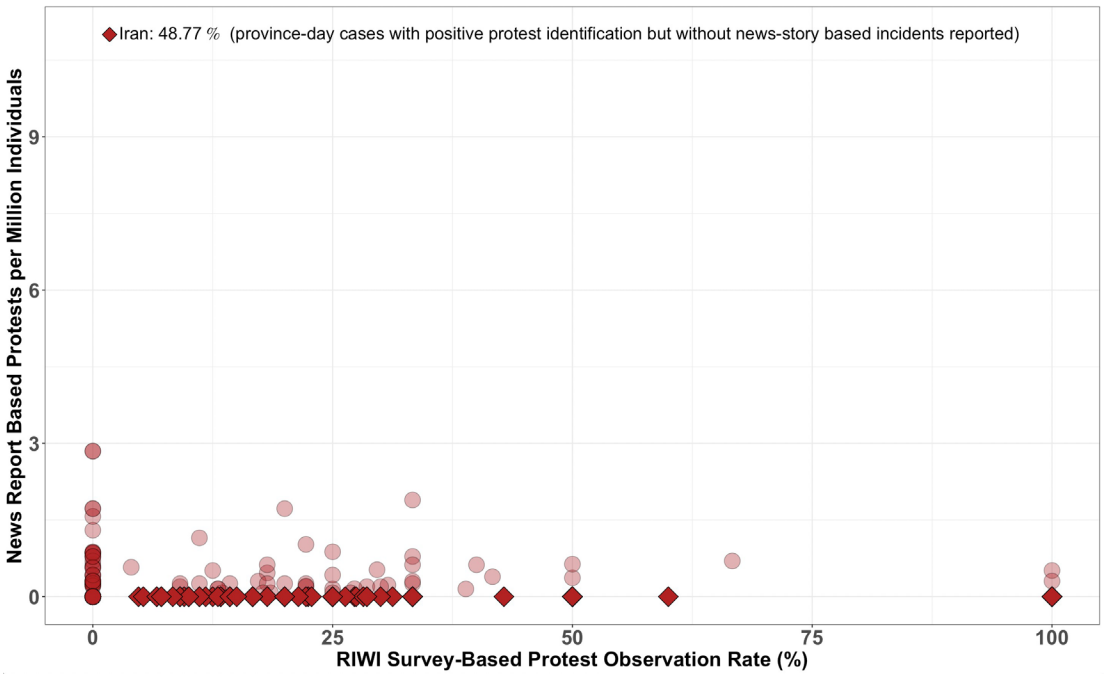
Figure 2. This figure displays survey-based first-level administrative boundary report rates of abductions and forced disappearance. (Darker shading represents greater report rates.) These results were collected between January 21, 2020 and July 6, 2020 across the countries of Cameroon, Chad, Niger, and Nigeria.



Finally, we analyze patterns of protest in Iran during a period of significant unrest between December 28, 2017 and January 07, 2018. The Dey Protests, initially centered around themes of economic hardship and government corruption, ultimately generated wider anti-regime sentiment, serving as a precursor to the 2018–2019 Iranian general strikes and protests. Iran's government responded by censoring social media and curtailing Internet access (Brocchetto and Andone, 2017). Given governmental efforts to suppress information flow, a critical question is whether the RIWI survey offers insights that leading media-based event data sets do not.

As with Afghanistan, results confirm the rapid manner in which the surveys can be launched for data collection. Critically, results (Figure 3) depict significant protest activity across province-days that are absent from the major conflict event data.

Figure 3. This figure displays reports of protest activity per province-day. Diamonds denote cases for which protest activity was reported by respondents but for which the news-report based data reported no activity: $\{(x, y) \in X | x > 0, y = 0\}$. Instances of positive protest reports where none are reported in the news-media based data make up approximately 49 percent of observations. See accompanying R code for additional details. (Plot uses data from acleddata.com.)



At issue, however, is the possibility that some (potentially significant) fraction of anonymous, online responses are spurious. Sargent et al. (2022) found that random domain intercept surveying provided COVID vaccination rate estimates similar to those from the CDC for national and state levels—evidence that the surveys may generally solicit legitimate responses. Yet, response quality in violent political settings is uncertain. This motivates our central empirical assessment of their validity detecting violence, unrest, and abuse.

3. Assessing Anonymous, Online Survey Efficacy

We sought to test whether anonymous, online surveys effectively bypass fundamental informational and editorial barriers to incident detection/reporting by running random domain intercept-based surveys in Bangladesh and Pakistan. We invited individuals who navigated to dormant websites (e.g. because they mistyped a web address) to participate in a survey.[15] This approach reaches a quasi-random sample of Internet users under the logic that errors associated with misaccessing websites are largely random across potential respondents. Not everyone will elect to participate in the survey once prompted, and various selection dynamics surely influence the ultimate set of survey results. We make no claim that the data are free from selection—as with virtually all other widely employed surveys where participation is necessarily voluntary. The survey technology used protects against bots and repeat takers—e.g. once a link is used, it cannot be re-accessed (by either the original user or anyone else with that link).[16] Potential respondents were informed that the survey is anonymous; they were free to answer or skip any questions or leave at any time, particularly if they were uncomfortable with any question; and they would not be asked for personally identifiable information.

After providing informed consent and reading about the research, participants were asked whether they had recently witnessed or otherwise had knowledge of a wide variety of events. Specifically, respondents were asked questions drawn from seven categories covering: (i) political violence and abuse, (ii) violence against civilians, (iii) violence against state forces; (iv) social unrest; (v) labor rights abuses; (vi) religion-related abuses; and (vii) property crimes or abuses. The full set of incidents about which they were asked appears in Appendix Section D.

The first category covered incidents of violent politics (e.g. abduction or forced disappearances of various political actors, unjust arrest(s) based on identity or political affiliation) and those related to the elections (e.g. violent voter intimidation, preelection threats or violence intended to restrict voting). Categories two and three related to attacks involving rape or other sexual violence, shootings, bombings, rock throwing, and mob violence, among various others.[17] Category four covered various types of social unrest or political demonstration including recent peaceful (and separately, violent) protests or demonstrations, riots, mob violence, et cetera.

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[15] This technology is offered by the RIWI Corporation. As RIWI (2023) describes, rather than "encountering a 'page does not exist' notification or ad, a RIWI survey or message test is rendered full site on the page … [potential respondents then] decide whether they would like to anonymously participate in the research."

[16] Please see Appendix Section C for additional details.

[17] In the case of violence against civilians specifically, before asking about violence type, we first asked about the targeted group—e.g. political or sociopolitical, religious, refugee, and other such groups.

The remaining categories asked about various labor, religion, and property-related abuses. Under labor, incidents spanned hiring discrimination or wrongful termination and, separately, lack of workers' rights, or infringement on workers' rights, both based on the ethnic, political, religious, gender, or other identity of the applicant. Under religion, incidents ranged from the murders of individuals committing *zina* (premarital sex) to honor killings to blasphemy charges being brought against an individual based on their religious beliefs or for statements made against their own or another religion. Similarly, under property, incidents spanned the destruction, arbitrary closure or takeover of local business(es) and widespread, organized vandalism.

The set of questions asked varied across respondents. For a large subset of incidents, we collected additional details (e.g. date, actors involved) from respondents that were shared with journalists to aid their verification efforts. This subset consisted of incidents we judged local community members (e.g. local law enforcement, human rights organizations, local media) to be aware of given the incidents' public nature (e.g. attacks on police, protests, arson) and, therefore, reasonably verifiable by journalists if they actually occurred. The subset was randomly selected conditional on it meeting this condition.[18]

To validate incidents reported through the surveys, we worked with professional journalists with experience reporting for major global news agencies on both countries and on the types of issues considered in this research. Utilizing the networks and methods inherent to their trade craft, they sought to confirm the authenticity of each incident shared with them.

Next, to determine whether anonymous, online surveys can be used to detect activity otherwise likely missed by existing collection methods, we compared relevant *validated* incidents against such datasets. Critically, many incidents—particularly relating to human rights abuses—are not presently tracked in incident-level datasets. Thus, in addition to contributing to a body of work that explores systematic missing in news report-based conflict event data, a major contribution of this study is to explore the possibility of establishing microempirical foundations (e.g. discrete spatial and temporal characteristics) of human rights abuses.

Below, we describe our country cases, survey design, and testing strategies.

[18] In contrast, we neither asked follow-on questions about, nor shared with journalists, details relating to incidents deemed highly personally and unlikely to be broadly known within the community—e.g. incidents of sexual violence. Our decision to exclude these events was based 1) on ethical concerns that the victims of such cases, however unlikely, might have been discovered by our journalists through their investigations and 2) the low likelihood that such events could be verified in the first place, otherwise producing significant numbers of false negatives and skewing results.

3.1 Country Cases

Our study focuses on the countries of Bangladesh and Pakistan, the world's eighth and fifth largest, respectively, by population (CIA, 2023a,b). In Appendix Section A, we offer a detailed discussion of the countries, noting key details here. Both are afflicted by contentious ethnic politics and significant violence, unrest, and abuse (ranking amongst the lowest in the world (Global Rights Project, 2023)). Journalists who report on these activities can face violence and retaliation (International Federation of Journalists, 2023; Human Rights Watch, 2021). Both have relatively low Internet penetration rates, ranking as the 18th and 48th lowest globally, respectively (ITU, 2022) and are accordingly hard cases for our study (King et al., 2021). They both host large refugee communities, which have been violently targeted. Both experienced violent, contentious elections during our study period, each with allegations of election tampering (Ellis-Petersen and Ahmed, 2024; ur-Rehman, Zia, 2024).

The countries also differ in key respects. Pakistan is victim to significant, long-running insurgency (Blair et al., 2013; Shapiro and Fair, 2010) and terrorism (Johnston and Sarbahi, 2016). In contrast, last year, Bangladesh experienced significant social unrest during the Student–People's Uprising (July Revolution), culminating in the ouster of the prime minister (Campbell, 2024). Violence reported during the period was substantial: "as many as 1,400 people may have been killed … and thousands were injured, the vast majority of whom were shot by Bangladesh's security forces … as many as 12–13 percent of those killed were children" (OHCHR, 2025b). Both countries have significant extreme weather and natural disaster risk and relatively low resilience levels (Institute for Economics and Peace, 2023) but differ significantly in terms of the patterns of weather and natural disasters affecting them.

3.2 Survey Design and Logic

Through the surveys, we 1) solicited responses to various questions related to violence, unrest, and abuse and 2) followed up on specific types of events, collecting additional details to aid journalists with verification efforts. We solicited journalist feedback during the survey's design to ensure that the event types and associated actors were relevant to local politics. Survey respondents were first asked to choose their preferred language between English (both Bangladesh and Pakistan), Bengali (Bangladesh), and Urdu (Pakistan). Respondents were then shown an IRB consent form; for those who did not give consent, the survey was ended.[19]

For verification purposes, a major focus was to identify precise details (e.g. dates, locations) associated with the randomly selected reported incidents. Doing so is challenging with anonymous, online surveying given high drop-off rates, particularly

[19] This survey excluded minors and those not residing in either country (regardless of citizenship).

when questions require more than single or multiselection (e.g. manually entering information).[20] To identify incident locations, we first identified the subnational locations in which respondents lived, asking about their division and district of residence.[21]

3.3 Soliciting Responses on Violence, Unrest, and Abuses

Next, we asked respondents about their knowledge of events across the seven categories described previously. Each respondent was randomly assigned a category, which included various specific incident types to choose from. In addition to category randomization, answer choice order was typically shuffled (see Appendix Section L). Respondents were then asked about their knowledge of the relevant events over the past two weeks.[22] For example, individuals randomly assigned to the social unrest category would have seen the text displayed in Figure 4.

If a respondent selected one or more listed events—e.g. in this example, selecting responses of "riot, mob violence, or other form of civil disorder... with police/government intervention" and "peaceful protest or demonstration with police or government intervention." One answer choice would be randomly selected, serving as the basis for follow-on questions. Continuing with this example, if the first response was randomly selected, the respondent would have then been asked which group(s) initiated the unrest, being shown a set of groups to select from including political, sociopolitical, religious, refugee, indigenous population, and student groups, amongst others. If they selected political, sociopolitical, and/or religious groups, in particular, given how significantly involved these groups are in Bangladeshi and Pakistani politics, additional questions would have been asked about the specific identities of those groups.[23] The exact nature of such follow-on questions varied across the categories but generally established what had occurred and which actors were involved. For instance, questions associated with violence against civilians concerned both the initiating group and targeted groups.

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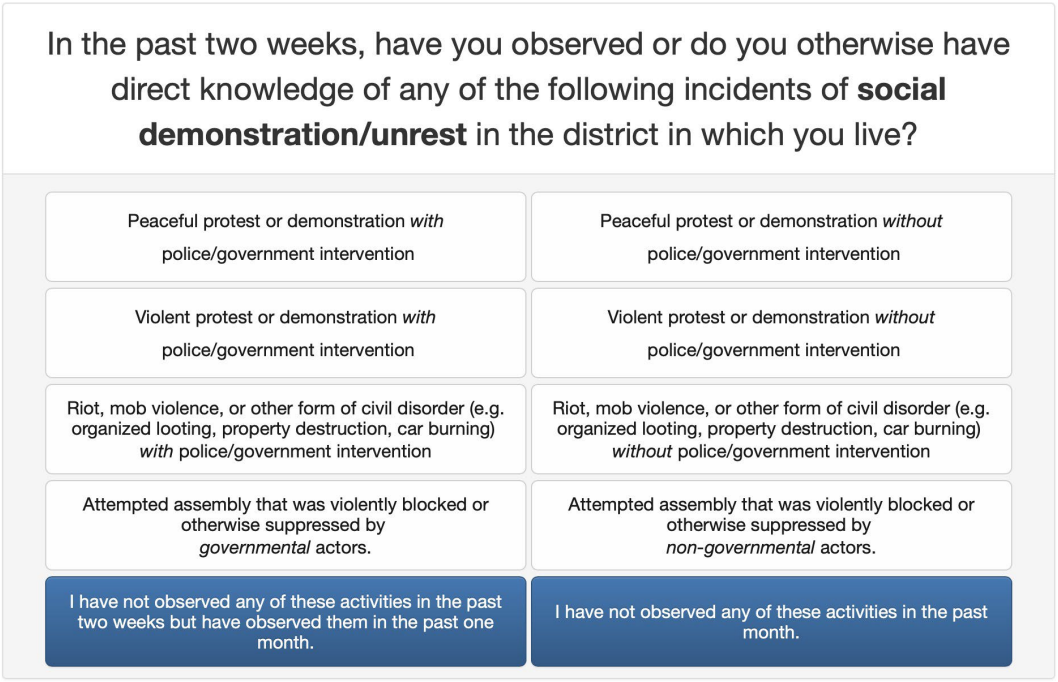
[20] RIWI strongly advised this approach given drop-off rates associated with previously tested alternative question types.

[21] Given the number of districts in both countries (64 in Bangladesh, 160 in Pakistan), asking for this information in a single question was impracticable. Ultimately, we were interested in the sub-districts in which events occurred. With 544 in Bangladesh and 577 in Pakistan, asking about these directly was not feasible.

[22] Asked about events over too short a period (e.g. the past day), respondents may have been unlikely to have anything relevant to share as we asked for direct observation or knowledge of events within a single district. Asking over too long a period (e.g. several months) would potentially introduce issues associated with partial recall of key details given decreases in memory (while also being more difficult for journalists to verify). Two weeks offers an approximate solution to this min-max optimization problem.

[23] For example, in the case of Bangladesh, Amar Bangladesh Party, Bangladesh Awami Swechchasebak League, and the Awami League, amongst other political groups, and Muslims (Ahmadi), Rohingya, Hindu (Brahmin), and Buddhist, amongst other religious groups.

Figure 4. This figure depicts what respondents randomly assigned the question about social unrest would have seen.



Finally, regardless of the question answered, the respondent would have been asked for two final details: the specific time period within the past two weeks during the event took place (today (the date on which the survey was conducted), 1–2 days ago, 3–4 days ago and so on through 13–14 days ago). Finally, we asked in which subdistrict the event took place. Once this process concluded, the respondent was asked about two more events, randomly selected from the remaining categories, and all relevant follow-on questions.

3.4 Journalist Verifications and Comparisons With ALCED

As surveys were completed, we shared key reported incident details with journalists. They engaged in their investigative processes to determine whether each event had occurred. Of the existing conflict event datasets, ACLED is kept up to date in near real time; tracks some human rights abuses; and cited more than competing data sets. Thus, we used it for our comparisons.

3.4.1 Novel Event Detection

Our first comparison involves exploring whether—and to what extent—the anonymous online surveys resulted in novel incident detection. We follow Shaver et al. (2023)'s approach to matching events against ACLED, which involves using the approximate locations (in our case, aggregating to the same sub-districts adopted in our surveys

(Humanitarian Data Exchange, 2020, 2022)), dates (aggregating to a several-day window), and event types and details to determine matches. The approach is favorable to ACLED as false positives are counted when they meet the inclusion criteria. To ensure an apples-to-apples comparison, we restrict those events reported by respondents and verified by the journalists to those also covered by ACLED. Specifically, our survey questions covered the event or sub-event types from ACLED listed in the footnote.[24]

Critically, our comparisons with ACLED are based on a *sample* of all ongoing events. We limited daily survey responses (about 50 per country) to avoid overwhelming the journalists' verification efforts. As we show, the overwhelming majority of verified events were not tracked by ACLED. Had our effort focused on identifying as many events as possible (i.e. imposing no response limit and focusing on collecting the universe of cases as the conflict-event datasets attempt), the total number of unique incidents would surely be far greater.

3.4.2 Detection Across Event Types

Our second primary analysis involves exploring how the survey and ACLED-reported events compare across event types captured. With a quota of about 50 daily survey responses, the anonymous online surveys provide a sample of events per day. Thus, to directly compare with ACLED, we proceed as follows: Let S_d denote the set of survey reported events per day that are comparable (i.e. mutually tracked) by ACLED. (Here, we focus on all reported events; not just those validated.) As survey questions asked about events that had occurred over the previous two weeks, let Ad represent the universe of mutually tracked ACLED events detected over [d−13, d]. We then generate m random samples $\mathbf{A}^*_{\mathrm{d}} = \{A_d^{*(1)}, \ldots, A_d^{*(m)}\}$, where each sample $A_d^{*(i)} = \{a_j\}_{j=1}^{n}$ consists of $n = |S_d|$ events drawn without replacement from A_d independently repeated $\forall\, i \in \{1, \ldots, m\}$.[25] Then, $\forall\, \mathrm{d} \in$ D, from the surveys, we calculate total reported cases of (i) peaceful protest, (ii) protest with intervention; (iii) violent unrest; (iv) political violence against civilians; and (v) politically driven property destruction. Finally, for each of the five categories, we calculate the mean count across all m samples of $\mathrm{A}*_{\mathrm{d}}$ along with the 2.5th and 97.5th percentiles that serve as uncertainty estimates. (See Appendix Section G.1 for details.)

[24] Protests, peaceful protests, protests with intervention, excessive force against civilians, riots, strategic developments, looting/property destruction, violence against civilians, sexual violence, abduction/forced disappearance, attacks, explosions/remote violence, grenade, drone strike, mob violence, battles, armed clash, non-state actor overtakes territory, and arrests.

[25] #bootstraps = 10,000.

4. Results

4.1 Reported Violence, Unrest, and Abuses

Respondents reported 15,227 total incidents (Bangladesh: 13,274, Pakistan: 1,953).[26] Survey descriptive statistics (see Appendix Section F) align with existing Internet user (gender, age, device type) demographics in both countries, further substantiating response quality.

4.2 Journalist Verifications

4.2.1 Overall Verifications

Of the 15,227 incidents reported, a total of 1,668 (893 from Bangladesh and 775 from Pakistan) were shared with journalists for further investigation and potential verification. (As a reminder, details of these events were requested at random from the broader set of reported incidents meeting the inclusion criteria for journalist verification.) For these, respondents supplied specific responses to the follow-on questions related to parties involved and specifically the sub-district in which the incident occurred. For Bangladesh, these cases came entirely from survey wave 2.[27] The spatial distributions of this subset of incidents are plotted in Figure 12.

For Bangladesh, between approximately 25.07 and 37.32 percent of all events anonymously reported were verified by the journalist. For Pakistan, these figures are approximately 54.93 and 55.73 percent, respectively. The smaller (larger) percentages represent cases in which we exclude (include) events identified by the journalists as potential but uncertain (i.e. partial) matches. For instance, a respondent reported widespread vandalism in Bangladesh's Kahaloo Upazila occurring between August 20–21, 2024. The journalist identified such an event falling outside of, but sufficiently close to, this date range, categorizing the event a partial verification. Results confirm that respondents frequently reported actual events. Whether the remaining events occurred but could not be unverified or were spurious (e.g. falsely reported) is unknown. We make two observations: first, the journalists' verification notes frequently describe lack of sufficient information for proper investigation but only rarely suggest that events were likely false. Second, to test for potential constraints on the journalists' verification efforts, we regress district population sizes along with estimated travel times from the countries' capital cities to the events in question using Google Maps Platform (2025)

[26] Some events may have been reported by multiple respondents. Though, recall that we asked about events occurring over the past two weeks and within their own district. So, each respondent's specific district-two week window would not have overlapped with most other respondents' specific district-two week windows.

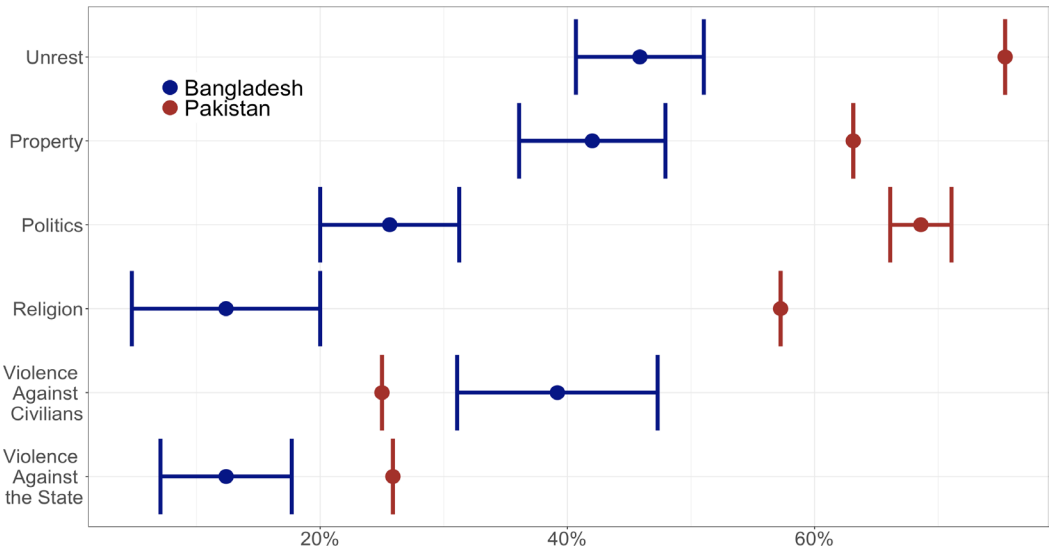
[27] Such incidents were also reported to the journalist with whom we were working on wave 1. However, this individual experienced a family emergency, preventing them from completing the work.

against a verification indicator.[28] Results for both countries depict fewer verifications in areas with larger populations—suggestive evidence that some nontrivial number of unverified events are likely false negatives (discussed this further in the conclusion in the context of possible improvements to future survey efforts.)

4.2.2 Heterogeneous Verification Patterns

Verification results depict meaningful heterogeneity when calculated across categories (Figure 5). While some results are consistent between the countries, others are not. Notably, social unrest verification rates are the highest for Bangladesh and Pakistan (approximately 47.59 and 75.41 percent, respectively). Rates associated with property incidents are similarly large: approximately 42.60 and 63.13 percent, respectively. In contrast, rates are the lowest for incidents of violence against the state, a pattern reflected across both countries. Yet, results for violence against civilians, religious abuses, and political violence or abuses show significant cross-country differences.

Figure 5. Journalist verification rates by category (means calculated from upper and lower bounds).



[28] In short, we model the relationship using logistic regression with category fixed effects. Complete details available in the accompanying R code.

4.3 Comparisons with Leading Event Data—Novel Event Detection

Matches with ACLED indicate that anonymous online survey responses detect a great deal of activity missed by prevailing conflict event data approaches. The overwhelming majority of incidents reported through the surveys and verified by journalists do not appear in ACLED.

4.3.1 Instances of Political Violence and Social Unrest

Our core set of comparisons involve all incidents of violence and unrest tracked by the journalists for which ACLED tracked the same class of events (e.g. abductions, kidnappings, shootings, in the case of political violence, and, e.g. protests, rallies, riots, in the case of social unrest). For Bangladesh (using data from both survey waves), of all relevant incidents of political violence and social unrest tracked by the journalists, we calculate that approximately 87.34 percent were not tracked by ACLED (lower, upper bounds of approximately 84.36 and 90.31 percent, respectively).[29,30] For Pakistan, this figure is approximately 88.04 percent [81.52, 94.57]. *Importantly, for both countries, rates of nonidentification are nearly identical when calculated.*

4.3.2 Instances of Human Rights Abuses

Comparisons with ACLED across cases of human rights violations are more difficult to make as ACLED does not systematically track a particular set of abuses. Instead, it tracks instances insofar as they as they aid in "understanding the context of conflict and disorder" for a given country at a given time.[31] Accordingly, "what types of events may be significant varies by context as well as over time, these events are, by definition, not systematically coded … [and such events] should not be assumed to be cross-context and time comparable."[32]

To proceed with ACLED comparisons across human rights abuses, we identify those that ACLED tracked in at least some cases (e.g. looting or property destruction) and then subset to comparable human rights incidents verified by the journalists. The following comparisons are, therefore, exploratory and should be interpreted with caution as ACLED may or may not have tracked all such incident types during our study period.

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[29] Lower and upper bounds hereafter expressed within "[]" for succinctness.

[30] For Bangladesh, specifically, within Wave 1, ≈84.93% [≈80.82%, ≈89.04%] of incidents of political violence and ≈90.10% [≈89.11%, ≈91.10%] of incidents of social unrest were not tracked by ACLED. Within Wave 2, these percentages are ≈86.56% [≈83.58%, ≈89.55%] and ≈86.70% [≈82.28%, ≈91.14%]. For Pakistan, we calculate a non-detection rates of ≈86.54% [≈84.62%, ≈88.46%] and ≈88.64% [≈80.30%, ≈96.97%].

[31] See https://acleddata.com/knowledge-base/strategic-developments-in-the-acled-dataset/what-are-strategic-developments-how-are-they-useful-and-how-should-i-use-them.

[32] See previous footnote for source.

In Bangladesh, during Wave 1 of the survey, we calculate that approximately 89.29 percent [85.71, 92.86] of potentially comparable human rights related events were not reported by ACLED. During Wave 2, that is estimate is approximately 94.95 percent [92.78, 100]. Finally, for Pakistan, that estimate is approximately 98.68 percent [97.34, 100].

4.3.3 Verified Events Circumvent Editorial and Selection Biases

As striking as these findings are, we can contextualize them further, considering how they compare to other detected events also not captured by ACLED. Besides verifying survey-reported events, the journalists also shared details of all of relevant events they otherwise learned about through their journalistic work—incidents that may or may not have ended up in news reports and ultimately into leading event datasets. Considering *capability* and *editorial* biases that afflict news report-based event data, some set of events are unknown to journalists and therefore not reported (e.g. those occurring in regions inaccessible to journalists). Others are known to journalists but ultimately omitted from reporting (e.g. those with fewer fatalities). Major conflict datasets will miss such events unless detected through non-news media channels.

These additional events tracked by the journalists were not impacted by constraints on *capability* (otherwise they would not have learned about them). Accordingly, they would be missed only if editorial biases led to their ultimate exclusion from major conflict event datasets. In contrast, the incidents reported by the survey respondents likely represent events excluded from such datasets for both capability and editorial reasons, with some number never having been known to journalists and some additional number being known but not published.

Thus, comparing the rates of ACLED coverage between the anonymous, verified incidents to the distinct events known to the journalists points to additional impact of *capability effects* on top of *editorial effects*. Overall, of these separate journalist-identified incidents of political violence and social unrest event types also tracked by ACLED, we calculate that in Bangladesh approximately 70.01 percent [65.20, 75.00] of incidents were not reported by ACLED.[33] (Recall that the estimated non-detection rate amongst verified survey incidents for Bangladesh is about 87.34 percent.)

[33] For Wave 1, these rates for incidents of political violence and social unrest are ≈68.89% [≈63.33%, ≈74.44%] and ≈80.15% [≈77.94%, ≈82.35%], respectively. For Wave 2, they are ≈46.43% [≈35.71%, ≈57.14%] and ≈75% [≈72.22%, ≈77.78%].

In Pakistan, approximately 46.40 percent [34.23, 58.42] were not tracked by ACLED (*vis-à-vis* nondetection of approximately 88.04 percent among verified survey-reported incidents).[34,35]

The significant but substantially lower non-detection rates amongst these separately identified incidents provide strong evidence of the survey method's effectiveness in unearthing activity otherwise likely to remain unknown to journalists and, therefore, ultimately to conflict event dataset curators and the scholars, government analysts and other users.

4.3.4 Anonymous Online Surveys and Human Rights Abuse Detection

A second notable theme in comparing incidents that the journalists learned about from the surveys and those they detected through other channels is variation across categories. Of all incidents reported by the journalists, they indicated learning about the greatest share amongst human rights incidents from the surveys, followed by incidents of unrest and, finally, violence.[36] These patterns suggest that anonymous online surveys might also be effective in surfacing specific incident types that are particularly unlikely to be detected by journalists.[37]

From a policy perspective, marginal returns to identifying particular event types may be especially great. For example, for early-warning efforts, detecting emerging patterns of abuse or violence against a given (religious, political, etc.) group may be more valuable than identifying attacks against a group already known to be targeted. Anonymous, online surveys may hold particular value in detecting activities primary sources (e.g. news media) are unlikely or slow to.

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[34] Specifically comparing incidents of social unrest, the rate is similar: ≈59.68% [≈51.61%, ≈67.74%] were not tracked by ACLED. Generating the same calculation for incidents of political violence, the rate is: ≈42.95% [≈29.88%, ≈56.02%] were not tracked by ACLED.

[35] We also calculate these rates for human rights abuses but with the caveats discussed above. For Bangladesh, Wave 1, we estimate that ≈82.05% [≈71.79%, ≈92.31%] events were not tracked by ACLED. For Wave 2, that is estimate is ≈75.00% [≈83.33%, ≈100%]. Finally, for Pakistan, that estimate is ≈80.61% [≈73.47%, ≈87.76%]. (Recall that these are relative to estimated nondetection rates for verified survey-reported incidents of ≈89.29%, ≈92.78%, and ≈98.68%, respectively.)

[36] Specifically, the percentages for Bangladesh are ≈71.35% (human rights abuses), ≈67.67% (social unrest), and ≈54.26% (political violence). For Pakistan, they are ≈60.8%, ≈51.56%, and ≈9.73%.

[37] Though, working with a relatively small set of journalists, these results are only suggestive.

4.4 Comparisons with Leading Event Data—Detection Across Event Types

Comparisons of event types reported by survey respondents and ACLED show markedly different patterns. Far from acting as substitutes, the two data generating processes appear to serve as strong complements (Figure 6).[38] ACLED's mechanisms for detecting social unrest (peaceful and violent) are robust; comparable samples show that ACLED captured far more of these events in Bangladesh with one exception: although protests with intervention were generally rarer, these cases of unrest were more likely to be reported by respondents. ACLED's sources (i.e. typically news media articles) perhaps less consistently report security force presence, making such cases more difficult to detect. For Pakistan, results are similar: ACLED captured many more peaceful protest cases; however, both collection methods fair similarly with respect to violent unrest. This does not mean (as our verifications show) they captured the same incidents—only that expected counts over equally sized samples are similar.

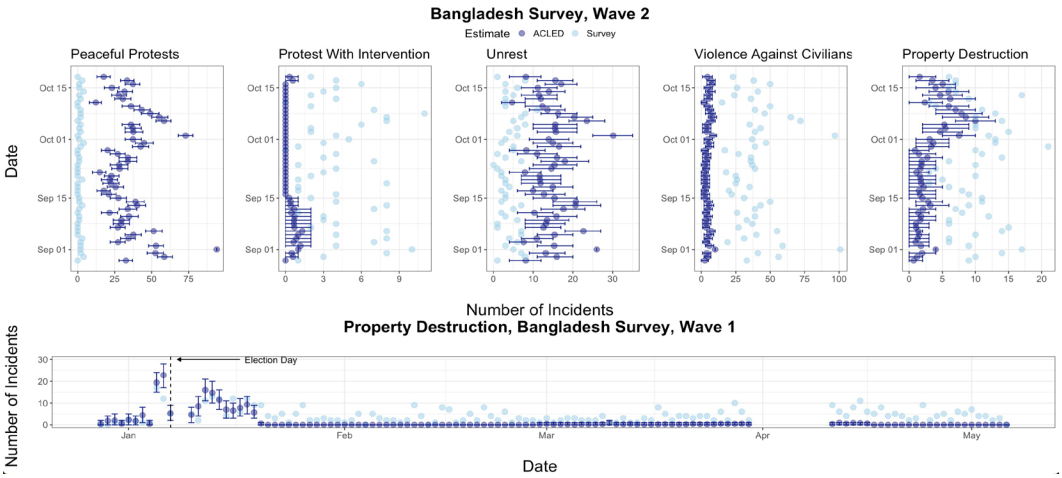
In stark contrast, the surveys capture much more politically motivated violence against civilians and property destruction in the countries. For instance, while both sources reported many cases of politically motivated property destruction immediately around the Bangladesh's 2024 election, ACLED's reporting drops off dramatically afterward while respondents depict substantial numbers of attacks for months thereafter, apparently confirming event dataset bias stemming from restricted media attention to events during politically salient periods.

As this analysis relies on all reported incidents, we consider how results would change were some number spurious or duplicates. The verification efforts substantiate the credibility of many reported incidents, likely undercounting true positives given the difficulties inherent to investigating thousands of reports across hundreds of millions of individuals. Nevertheless, two major checks substantiate robustness. First, we assume half of all events reported were spurious (halving the daily number of events per type reported), and compare remaining incidents to ACLED. Separately, we adopt a highly conservative approach that assumes *all* events of a given type reported within 14 days of one another in the same second-level administrative unit are the same event.[39] In both cases, differences attenuate as expected but are substantively unchanged.

[38] We display several representative results in the paper; remaining estimates are in Appendix Section B.

[39] This approach uses a form of greedy algorithm whereby, considering events in reverse chronological order, like events are eliminated. Please see the associated R code for complete details.

Figure 6. This figure displays the results of comparing daily survey reported incidents with equally sized samples of ACLED events. Anonymous, online surveying and (primarily) news-report based collection methods tend to detect different incident types, suggesting that two are complementary and more effectively approximate actual patterns of violence and unrest together than on their own.



4.5 Patterns of Human Rights Violations

Given the dearth of incident-level human rights data to compare against survey responses, we instead ask how the trends and patterns the responses depict compare to those presented by reports like Amnesty International's *The State of the World's Human Rights*. At the time of writing, some of these reports were not yet released—underscoring further the value of anonymous, online surveys in rapidly surfacing information. Two major reports are available: HRW (2025)'s "World Report 2025" and OHCHR (2025a)'s "Human Rights Violations and Abuses Related to the Protests of July and August 2024 in Bangladesh", which explore alleged abuses perpetrated during or after the period of unrest culminating in revolution.

A clear difference between the survey responses and reports is lack of attention by the latter to issues that do not fit within specific salient political themes. Bangladesh's period of revolutionary unrest and immediate aftermath are the clear focus of HRW (2025). Yet, some of the most frequently reported issues by survey respondents are largely independent of that context. For instance, religiously related abuses, including forced marriages, honor killings, forced conversions, and the execution of individuals for engaging in sexual relations outside of marriage, are amongst some of the most reported issues.

Lack of reporting does not imply that these organizations were unaware of these issues (though their focus elsewhere might have limited their awareness); however, just as incidents of violence or unrest not reported by journalists are unlikely to feature in news report-based data sets, neglecting such issues threatens to skew major human rights datasets dependent on these reports—and, by extension, the academic inferences gleaned from them.

A second example are reports of violence against the community of sex workers in both countries. Strikingly, such violence was the fourth and eighth most commonly reported event in the Bangladesh and Pakistan surveys, respectively. There is no reference to such activity in HRW (2025), and references to violence faced by such communities outside of major reports are rare and typically within broader health focused (e.g. HIV) contexts (Mayhew et al., 2009).

Of equal note is lack of attention to Bangladesh's contentious elections, save for a reference to arrests of opposition members or supporters. Restricting responses to those received between the period spanning December 28, 2023 through one month after the elections, respondents reported many hundreds[40] of instances of violence against students, political and religious groups, torture, arson, mob violence, and lynchings. While subsequent events that year might seem to render election events less relevant (there is, indeed, a new government in place now), lack of attention threatens to skew data and inferences as the absence of information in such reports is construed as lack of violence and abuse altogether.

5. Robustness to Internet Connectivity

5.1 Pakistan's Suspension of Cellular Communications

An unexpected natural experiment occurred during our survey period when the Pakistani government suspended cellular telephone communications on the election day (Amnesty International, 2024). Von Borzyskowski and Wahman (2021) document significant underreporting on electoral violence in media-based conflict event datasets, and such communications outages threaten to conceal much election-day violence, unrest and abuse. We find that, despite the mobile network suspension, the flow of survey responses was unaffected (See Figure 7). It appears that sufficient responses over broadband offset effects of the outage.[41]

[40] As this figure is based on daily samples, the actual number is surely much larger.

[41] We set a daily quota of ≈50 responses. The larger number of responses ahead of the election do not show a reduction in the flow of responses but initial variance as RIWI targeted that daily rate.

5.2 Broader Tests of Connectivity on Survey Initiations

Given anonymous, online surveys' dependence on Internet connectivity, we explore the relationship between mobile and fixed broadband connections during the survey periods and the number of individuals who initiated the survey across both countries' second-level administrative units.[42] Individuals may elect to not complete a survey for various reasons correlated with local connectivity. However, cases of individuals initiating the survey, regardless of whether they provided substantive answers thereafter, measures *ability* to connect.[43]

Specifically, we calculate for each quarter-administrative unit the weighted average download and upload speeds for both mobile and fixed broadband Internet using global, granular data (Ookla, 2025). The distribution of speeds by type across both countries' administrative units are plotted in Figure 8. Using polynomial regression, we regress these speeds against the number of individuals who initiated the survey.[44] As the outcome variable is a count, we generate results with both ordinary least squares (OLS) and Quasi-Poisson regression. For the latter, we generate expected counts across the range of observed speeds within the 2.5th and 97.5th percentiles and generate uncertainty estimates using Quasi-Bayesian Monte Carlo simulation. Areas with more individuals would reasonably be expected to generate larger response numbers, and more densely populated areas may enjoy higher rates of connectivity. Thus, we control for population size in each administrative unit (Wikipedia, 2025; Geo-Ref.net, 2024). We also supplement the model with quarter and mobile versus fixed broadband fixed effects.

Connectivity effects on initiation numbers are mixed (Figure 8), with no clear impact in Bangladesh and increasing initiation in Pakistan at higher speeds.[45] For Pakistan, a one standard deviation (σ) increase in weighted mean download (upload) speeds from the mean (approximately 15k kilobytes per second (kbs^{-1}) (≈10.5 kbs^{-1}))) is associated with approximately 8.55 (≈5.46) additional initiations. Fortunately, sampling more intensively from such areas offers a direct solution to cases in which detected initiations are

[42] We adopt the 2nd level administrative unit as the number of Ookla (2025) tests conducted per grid cell can be limited. Aggregating tests to the second-level administrative unit ensures large samples per administrative-unit quarter, obviating possible extreme results that might otherwise result from small samples.

[43] To match survey initiations to respondent locations and subsequently to internet speeds, we require respondents' districts, which come from the third survey question. Thus, our analysis is based on those individuals who initiated the survey and proceeded to answer these first few questions, which were posed before any substantive questions (and project description) were presented.

[44] We limit higher degree terms to the cubic term to avoid possible over-fitting. We present results for which uncertain estimates remain within reasonable limits.

[45] Despite differences in country average speeds, completion rates between Bangladesh and Pakistan were nearly identical. When we replicate the regressions described, substituting initiation counts with survey completions, we find no statistically significant relationship with internet speeds. See accompanying R code.

systematically lower. Thus, to ensure geographic representation, researchers might assess *ex ante* per capita rates of initiation to inform final sampling procedures.

Figure 7. This figure displays total daily survey responses collected in Pakistan with no noticeable effect on information secured immediately before, during, or after the election despite a government shut down of cellular communications.

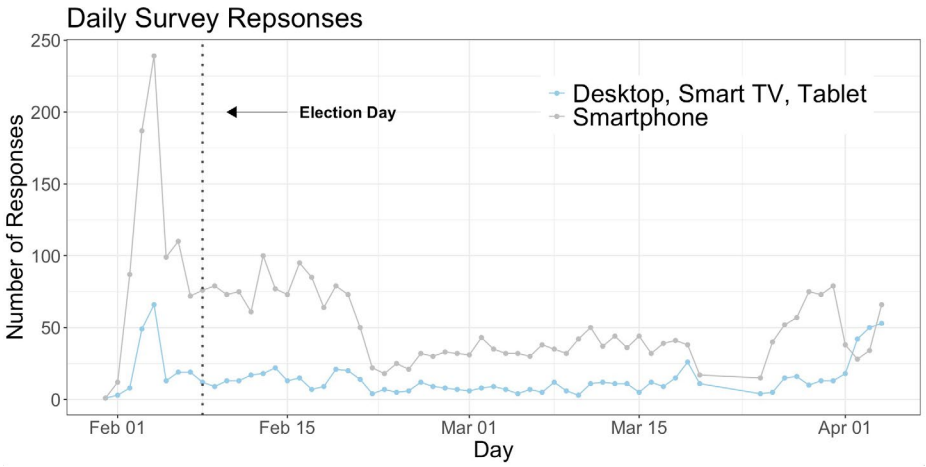
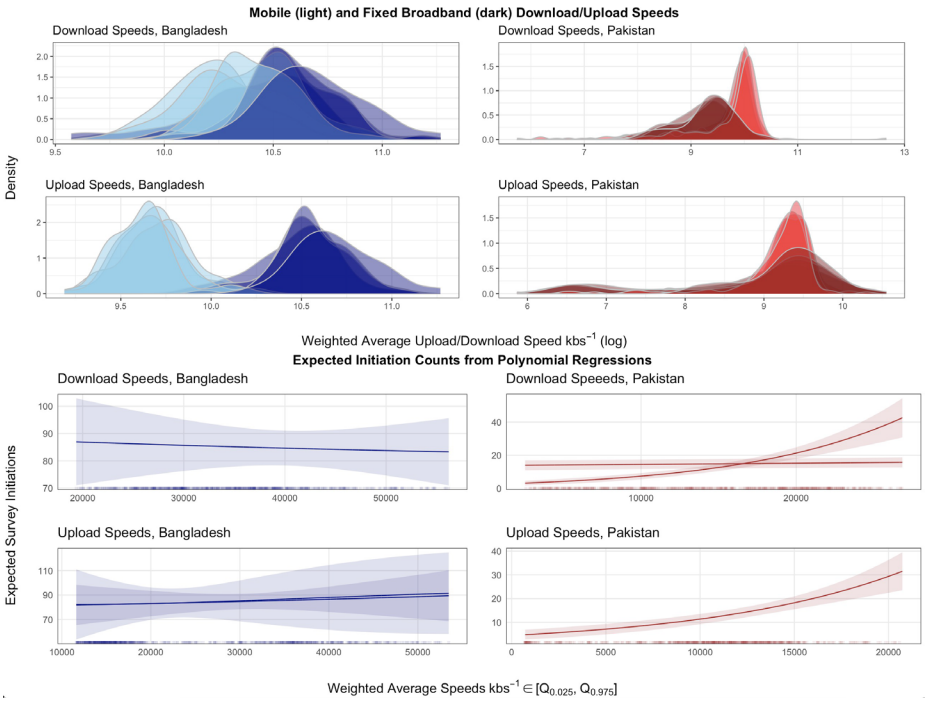


Figure 8. Top: Distributions of weighted average mobile and fixed broadband Internet (download/upload) speeds across Bangladeshi and Pakistani districts for each quarter of 2024 estimated from speed tests across grid cells globally. Bottom: Expected survey initiation counts from polynomial count model regressions. Sources: Authors; Ookla (2025).



6. Discussion and Conclusion

We find that anonymous, online surveys are effective in detecting otherwise unobserved incidents of political violence, social unrest, and human rights abuses in Bangladesh and Pakistan. The verification estimates we calculate are likely lower bounds of actual rates, and the prospects of anonymous online surveying are likely more significant than we establish. First, our approach relied on four professional journalists to investigate thousands of events that took place across countries with hundreds of millions of individuals spread across hundreds of thousands of square kilometers. It follows that some, potentially significant, number of cases did indeed occur but could not be confirmed given practical limitations.

Relatedly, our approach involved identifying events at the level of the sub-district. This level of spatial granularity helpfully narrowed the journalists' focus within administrative regions with relatively small populations (e.g. Gishkore, Pakistan, population approximately 20,000). However, some subdistricts alone are home to 1 million-plus residents (e.g. Islamabad, Pakistan). Retrospective discussions with the journalists made clear that verification efforts were considerably more challenging in such areas, further increasing the likely incidence of false positives.

Finally, high verification rates across multiple event types clearly indicate respondents' willingness to share details about actual events. Lower verification rates across some categories (e.g. physical assault, which is so pervasive, identifying individual cases proves difficult) may relate less to response quality than verification challenges. For example, whereas protest details may be known to multiple sources, attacks on state forces might be known and therefore verifiable only by those forces, which may sequester details.

Nevertheless, rates could be inflated by incidental discoveries of like events. While looking for evidence of events that occurred at particular locations and times, the journalists may have discovered similar events similar. Such cases are likely to be limited given the variety of event details (nature, date, location, actors involved) shared with the journalists to aid their efforts. Still, we cannot rule them out entirely.

How might scholars and data curators effectively engage in future anonymous, online surveying? First, we note that some limitations we encountered are not necessarily inherent to anonymous, online surveys but specific to the particulars of this effort. For instance, where researchers identify lower initiation rates (whether stemming from technical, cultural, or other issues), they might work to adjust sampling procedures accordingly. However, some issues are inherent to the broader methodology.

For instance, by design, the anonymous nature of the surveys, while optimizing respondent safety, makes call-back engagement impossible, precluding some research designs (e.g. assessing lagged treatment effects).

Regarding event location, there are limitations to identifying events within insufficiently small (physically or by population) administrative boundaries. However, when we explored asking respondents about the location of events at units below the third-level administrative boundary, we encountered standardization issues; how respondents describe by name local neighborhoods, boroughs, et cetera (and which boundaries those names describe) can vary significantly across both regions and respondents residing within them.

Furthermore, although we attempted to capture a broad set of potential activities, when asked about some activities (e.g. violence against civilians), many respondents reported "other." For instance, when asked about the group being targeted, although we listed eleven possible responses, many opted for "other," and they did so similarly when asked about type of violence (among a large battery of choices predetermined in consultation with the journalists). Additional survey pretesting is likely to help researchers and data curators improve response quality.

Anonymous, online surveying has broader applications, particularly where time sensitivities and/or difficult-to-reach demographics are involved. In the immediate aftermath of natural disasters, these methods may prove useful to humanitarian relief efforts requiring rapid need assessments.[46] In contexts in which new outbreaks of disease, violence, etc. are suspected, this method might facilitate early detection efforts that seek to limit spread early on.

With respect to survey participation and respondent protection, future research might also consider: the sampling properties random domain intercept technology,[47] how the manner in which anonymous, online surveys are introduced to potential respondents influences participation,[48] and whether recent scientific discoveries about re-identification threaten random domain intercept surveying (Evans et al., 2022). Scholars might also explore the generalizability of our results to countries beyond Bangladesh and Pakistan.

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[46] When Internet and/or electricity are unavailable, the surveys might reach individuals within geographically proximate communities who may have emerging information about their neighbors.

[47] Do systematic differences exist between those populations that do and do not reach surveys in spite of the quasi-random nature of survey assignment? Amongst the former, how do those who participate differ from those who decline?

[48] For example, would respondents have engaged differently had our project not been associated with the University of California, with social science research?

External validity is partially confirmed by RIWI's successful execution of similar projects within other regions of the world (Afghanistan, Iran, Lake Chad Basin region). Yet, there may be important questions to re- solve relating, for instance, to appetite for expression. Although citizens living under repressive regimes might avoid participating for fear of detection by authorities, the opposite might also prove true; with limited opportunities to make their voices heard, the surveys may offer a unique outlet for expression.

Finally, anonymous, online survey use to detect violence, unrest and abuse has so far occurred on limited geographic and temporal bases. If broadly adopted, it would be crucial to assess whether continuous survey deployments result in survey fatigue (or, per the bystander effect, reduced reporting as potential respondents presume others are reporting) and, critically, possible strategic retaliation by state (or other political) actors seeking to suppress reporting.

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Appendix

Contents

A. Country Cases: Bangladesh and Pakistan

This study focuses on the countries of Bangladesh and Pakistan, which house the world's eighth and fifth largest populations, respectively (CIA, 2023a,b). Both countries are subject to significant political violence, social unrest, and human rights abuses. Journalists attempting to report on these activities face violence and other forms of retaliation (International Federation of Journalists, 2023; Human Rights Watch, 2021). In the following section, we highlight similarities and differences between the two countries which motivated their selection as case studies.

First, both countries have relatively low Internet penetration rates (21 percent for Pakistan and 39 percent for Bangladesh), respectively ranking as the 18th and 48th lowest rates globally (ITU, 2022). The low penetration rates challenge our methodology (King et al., 2021), allowing us to show that anonymous, online surveying can indeed be effective in capturing otherwise hidden violence and persecution. Thus, the success of our methodology confirms that it would be well suited in a majority of countries where access to the Internet is at least as great. While both countries encounter low connectivity, their penetration rates are different enough to infer how differing levels may affect the effectiveness of online surveying.

Second, they both experience high levels of ethnic conflict, connecting them to a broader field of interest (Manekin and Mitts, 2022; Sambanis and Shayo, 2013). Additionally, both are subject to significant, ongoing human rights abuses, perpetrated by both state and subnational actors, ranking amongst the lowest in the world's human right scores according to the Global Rights Project (2023).

Third, both house large refugee communities, which have faced significant violence while displaced. In Bangladesh, Rohingya refugees, numbering 1,139,433 as of 2025 (UNHCR Operational Data Portal, 2025), having fled persecution in Myanmar, face new attacks in their asylum country (Tan, 2023). Similarly, Pakistan hosts millions of refugees, with the majority having fled from Afghanistan (Global Focus, 2024). Eight hundred thousand Afghan refugees in Pakistan are estimated to have either voluntarily or forcibly returned to Afghanistan (Ahmed, 2025), with new deportation plans that could more than double this number (Hussain, Abid, 2025). Thus, this study converges with a developing body of work on violence against refugee populations (Gineste and Savun, 2019; Savun and Gineste, 2019).

Fourth, both Bangladesh and Pakistan experienced contentious elections during the surveying period. In Bangladesh, elections were held on January 7, 2024 (AP, 2023). In Pakistan, after being delayed, they were finally held on February 8 (Qayum and Haider, 2023). Both countries underwent significant violence and social unrest preceding, during, and in the aftermath of their elections (Tyab, Imtiaz, 2024), including allegations of election tampering (Ellis-Petersen and Ahmed, 2024; ur-Rehman, Zia, 2024). In Pakistan, cellular telephone service was suspended during the elections (Tyab, Imtiaz, 2024). This study, therefore, provides opportunities to explore electoral violence dynamics (Hafner-Burton et al., 2014, 2018; Young, 2020), which have been historically limited by media reporting biases (Von Borzyskowski and Wahman, 2021).
Yet, the countries also vary in key aspects, providing for the exploration of important differences. Pakistan, for instance, is victim to significant, long-running insurgency (Blair et al., 2013; Shapiro and Fair, 2010) and terrorism (Johnston and Sarbahi, 2016)– such as Baloch insurgents actively engaging government forces (Azam, 2020). Indeed, Shaver et al. (2023) recently found that insurgency in Pakistan's Balochistan province is far more significant than reported by the news media, fueled, in apparent part, by arms smuggled into the country from Afghanistan following the United States' withdrawal.

Furthermore, Pakistan is also the only country with nuclear weapons for which substate militancy proves a considerable risk (Kapur and Ganguly, 2012). As Bueno de Mesquita et al. (2015) note, "the extensive presence of non-state violent actors perennially stokes fears that one of these groups will acquire nuclear materials or technology."

In contrast, last year, Bangladesh experienced significant social unrest during the July Revolution, culminating in the ouster of then-Prime Minister Sheikh Hasina on August 05, 2024 (Campbell, 2024). Violence reported during the period was substantial. The UN Human Rights Office reports that "as many as 1,400 people may have been killed between 15 July and 5 August, and thousands were injured, the vast majority of whom were shot by Bangladesh's security forces... [A]s many as 12-–3 percent of those killed were children" (OHCHR, 2025).

B. Additional Results

Figure 9. This figure displays the set of all incidents reported by respondents in Bangladesh having been asked about activity specific to the district in which they live and for events occurring over the past two weeks.

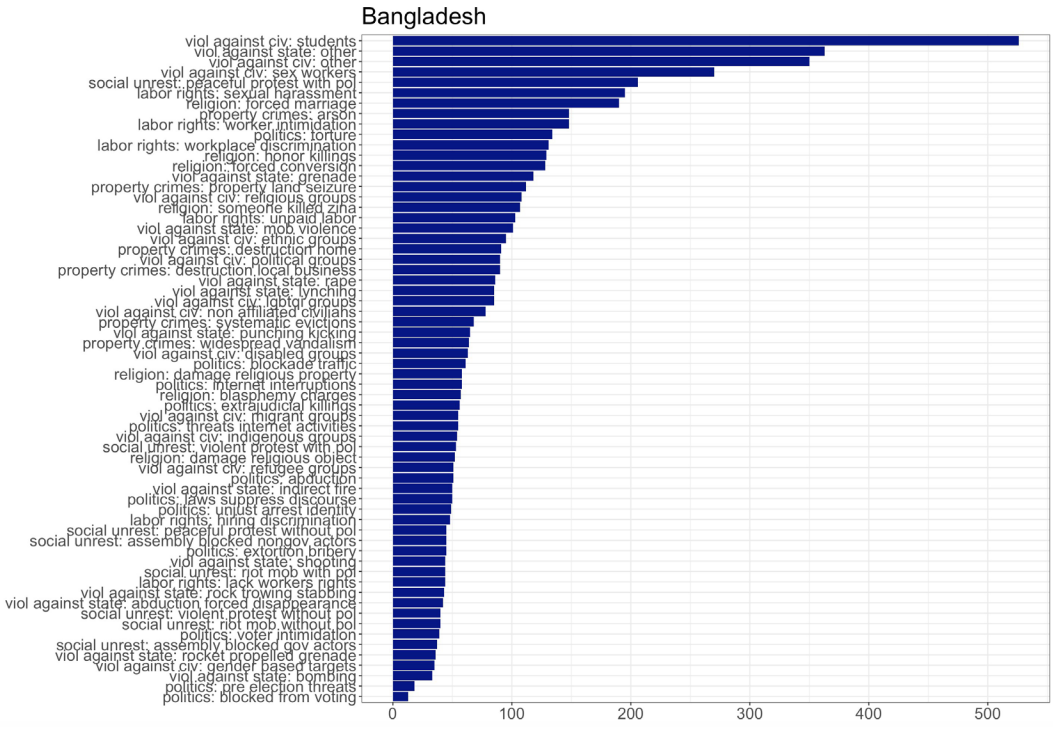


Figure 10. This figure displays the set of all incidents reported by respondents in Bangladesh having been asked about activity specific to the district in which they live and for events occurring over the past two weeks

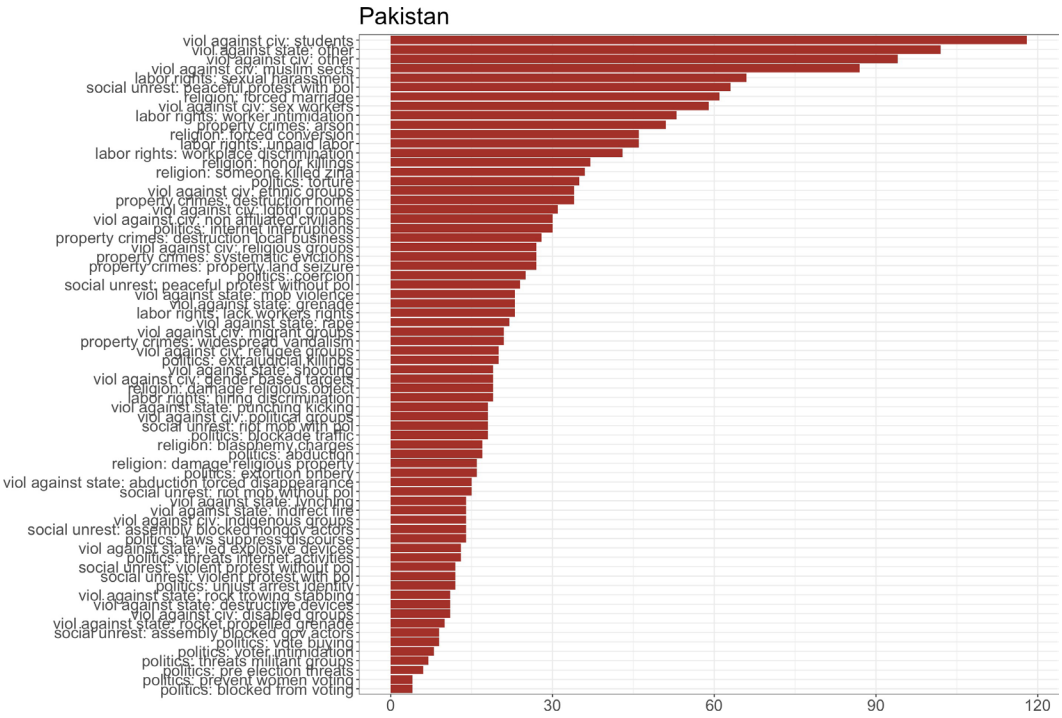


Figure 11. This figure displays the geographic distribution of the subset of incidents reported by survey respondents whose details were shared with the journalists for potential verification.

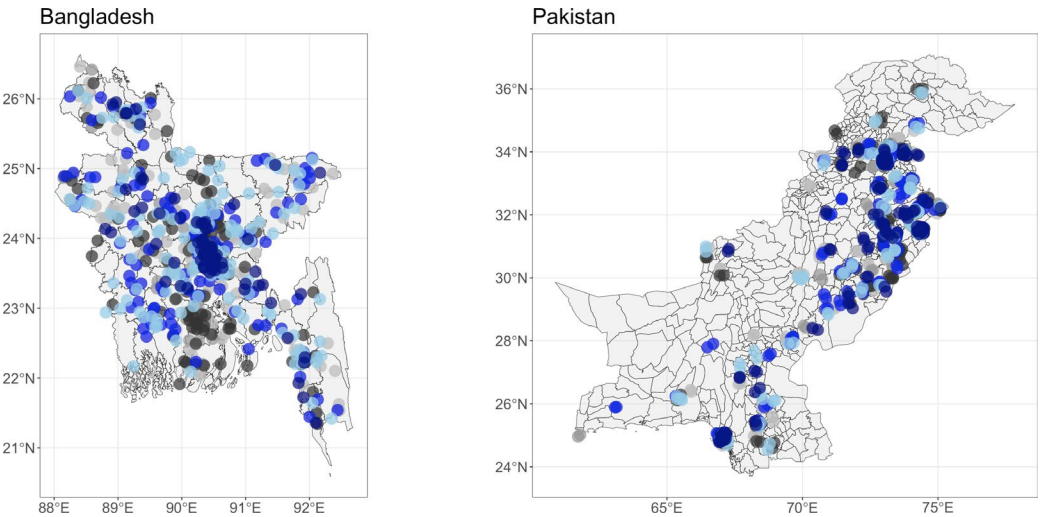


Figure 12. This figure displays the frequency of incidents shared with the journalists for potential verification reported by respondents.

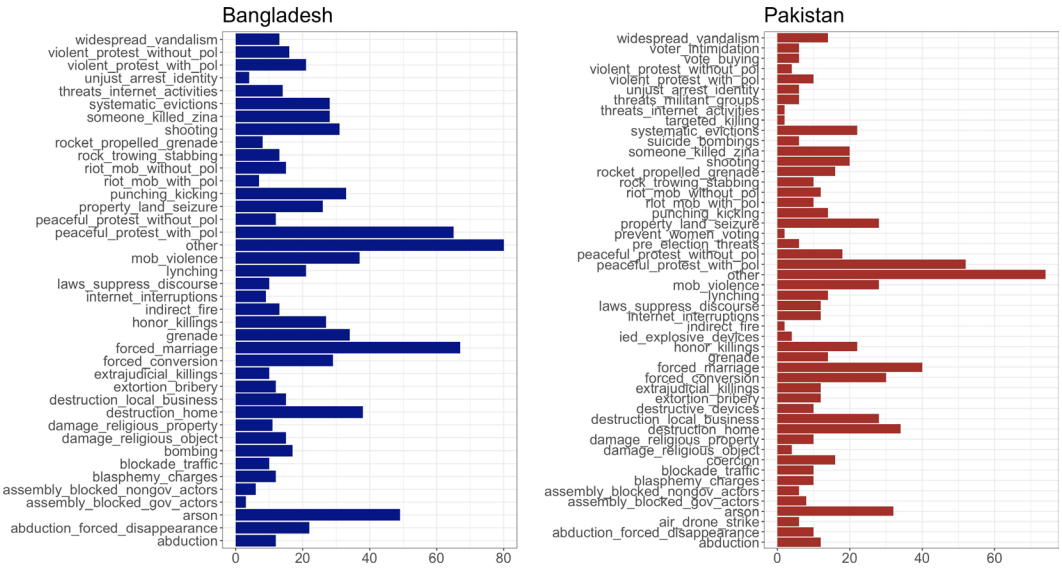


Figure 13. This figure displays the results of comparing the daily set of survey reported incidents with equally sized samples of ACLED events for the first survey wave in Bangladesh.

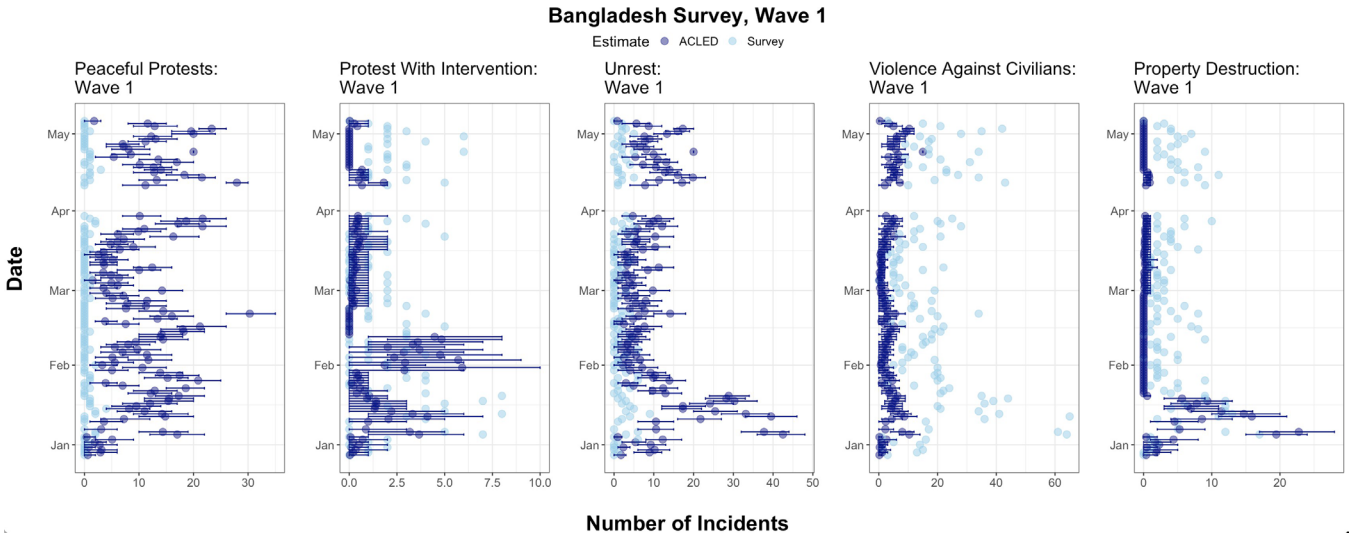
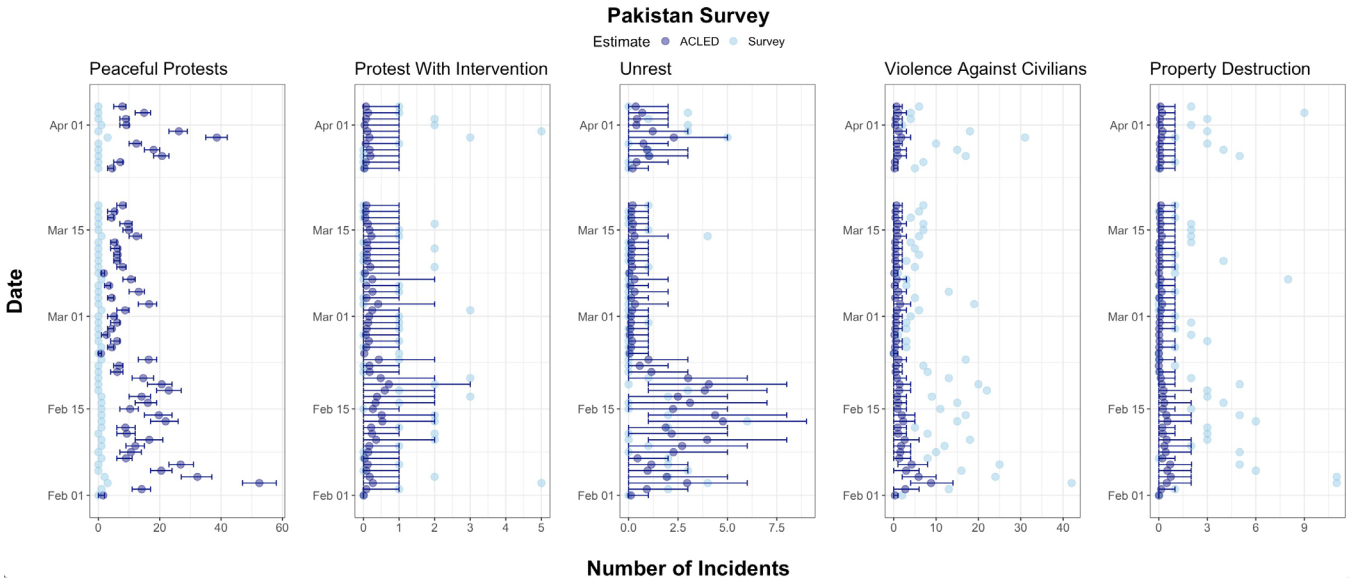


Figure 14. This figure displays the results of comparing the daily set of survey reported incidents with equally sized samples of ACLED events for the survey in Pakistan.



C. Additional Details About RIWI's Procedures for Ensuring Survey Integrity

The survey technology used protects against bots and repeat takers—e.g. once a link is used, it cannot be re-accessed (by either the original user or anyone else with access to that link). RIWI supplied the following details for readers given that the technology in central to the integrity of the results generated: "1) **Preventing retakes/re-accessing:** RIWI ensures that respondents are unique and have not previously completed the survey by using multiple security layers, including continuous bot filtering and anomaly detection. Additionally, entry points to the survey (i.e., lapsed web domains) rotate regularly, preventing individuals from returning via the same path. 2) **First-time survey access and return attempts:** Respondents cannot proactively seek out the survey (e.g., there are no ads or centralized entry points). They are randomly intercepted while browsing online. If they attempt to return to the same link, they will typically be redirected elsewhere or blocked from re-entering. 3) **Accessing from another device:** If someone tries to access the survey again from a different computer or network, they will not be able to re-enter. Once a participant lands on the survey link—whether they complete it or not—the access is broken, preventing any further attempts from the same or another device. Additional details: RIWI's technology minimizes biases such as self-selection, social desirability, acquiescence, and incentive bias. It also continuously filters out bots and ensures data authenticity through real-time anomaly detection techniques like straight-lining detection."

D. Primary Survey Questions and Answer Choices by Category: Bangladesh

In the following questions, answers were presented in a randomized order unless otherwise denoted by an asterisk (*). This randomization does not apply to answer options that do not give substantive information (e.g., “Other,” “I don’t know,” “I have not observed [activity] in the past month.”). Additionally, all questions enabled multiselection unless otherwise indicated by a dagger (†).

D.1 Social Unrest (SU)

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of social demonstration/unrest in the district in which you live?*

1. Peaceful protest or demonstration with police/government intervention
2. Peaceful protest or demonstration without police/government intervention
3. Violent protest or demonstration with police/government intervention
4. Violent protest or demonstration without police/government intervention
5. Riot, mob violence, or other form of civil disorder (e.g. organized looting, property destruction, car burning) with police/government intervention
6. Riot, mob violence, or other form of civil disorder (e.g. organized looting, property destruction, car burning) without police/government intervention
7. Attempted assembly that was violently blocked or otherwise suppressed by governmental actors
8. Attempted assembly that was violently blocked or otherwise suppressed by non-governmental actors
9. I have not observed any of these activities in the past two weeks but have observed them in the past one month.
10. I have not observed

D.2 Violence Against Civilians (VAC)

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of violence against any of the following groups in the district in which you live (this violence could be civilian-on-civilian, government-on-civilian, non-state actor-on-civilian, etc.)?

1. Political/Socio-political Groups
2. Religious Groups
3. Refugee Groups
4. Migrant Groups
5. Gender-based Targets (violence against women because they are women, etc)
6. Indigenous Populations
7. Disabled Populations
8. LGBTQI+ Populations
9. Sex workers
10. Ethnic Groups
11. Students
12. Non-affiliated civilians
13. Other
14. I have not observed violence against the groups in the past two weeks but have observed them in the past one month.
15. I have

Question: What act(s) of violence were committed against [THE GROUP from Q8]?

1. Rape or other form of sexual violence
2. Shooting (e.g. pistol or rifle attack)
3. Bombing (e.g. improvised explosive device attack, land mine)
4. Indirect fire (e.g. rocket attack, mortar attack)
5. Grenade
6. Rocket-propelled grenade
7. Rock throwing, stabbing or other non-explosive weapon
8. Punching, kicking, or other form of violence without weapons
9. Abduction/forced disappearance
10. Mob violence
11. Lynching
12. Other
13. I don't know

D.3 Violence Against the State (VAS)

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following cases of violent attacks against state forces (e.g. an attack against military, police, personnel, etc.) in the district in which you live?

1. Rape or other form of sexual violence
2. Shooting (e.g. pistol or rifle attack)
3. Bombing (e.g. improvised explosive device attack, land mine)
4. Indirect fire (e.g. rocket attack, mortar attack)
5. Grenade
6. Rocket-propelled grenade
7. Rock throwing, stabbing or other non-explosive weapon
8. Punching, kicking, or other form of violence without weapons
9. Abduction/forced disappearance
10. Mob violence
11. Lynching
12. Other

D.4 Labor Rights

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following cases of labor rights abuses (e.g. unpaid labor, hiring discrimination, etc.) in the district in which you live?

1. Unpaid/underpaid Labor
2. Hiring discrimination or wrongful termination based on the ethnic, political, religious, gender or other identity of the applicant.
3. Lack of workers' rights, or infringement on workers' rights based on the ethnic, political, religious, or gender identity of the applicant.
4. Worker intimidation
5. Sexual harassment
6. Workplace discrimination
7. I have not observed any cases of labor rights abuses in the past two weeks but have in the past month.
8. I have not observed any cases of labor rights abuses in the past month.

D.5 Religion

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of violence against members of a religious group attracting broad public attention in the district in which you live?

1. Someone killed for committing Zina (premarital sex)
2. Blasphemy charges being brought against an individual based either on their religious beliefs, or for statements they have made against their own or another religion
3. Damage to or destruction of religious property (e.g. a mosque, church, temple)
4. Damage to or destruction of religious object (e.g. a bible, hijab)
5. Forced conversion
6. Forced marriage
7. Honor killings
8. I have not observed any cases of violence against members of a religious group in the past two weeks but have in the past month.
9. I have

D.6 Politics

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of violence related to political affiliation, political tensions, or election-related tensions in the district in which you live?

1. Extortion/bribery of government, police, political party officials, etc.
2. Abduction/forced disappearances of government officials, political party members, or civilians for political reasons
3. Torture
4. Extrajudicial killings
5. Violent voter intimidation
6. Being blocked or restricted from voting at or immediately around voting/polling stations
7. Pre-election threats or violence intended to restrict voting
8. Charged or threatened with charges (blasphemy, libel, slander) due to internet activities
9. Internet/telecommunication interruptions/outages due to heightened political activity (elections, protests, etc.)
10. Blockade to restrict traffic or commerce
11. Unjust arrest(s) based on identity or political affiliation
12. Use or threaten to use of libel/slander laws to suppress political discourse
13. I have not observed any politically motivated violence in the past two
14. weeks but have in the past month.
15. I have not

D.7 Property Crimes

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of property crimes towards someone of a specific religious, ethnic, gender, and/or political identities?

1. Property/Land Seizure
2. Destruction or invasion of home/farm
3. Destruction, arbitrary closure or takeover of local business(es)
4. Systematic unlawful evictions
5. Widespread, organized vandalism of property
6. Arson
7. I have not observed any incidents of property crimes in the past two weeks but have in the past month.
8. I have not

D.8 Gender

Question: In the past year, have you observed or do you otherwise have direct knowledge of any of the following incidences of gender-based violence resulting in public attention in the district in which you live?

1. Forced marriage
2. Forced sterilization
3. Sexual harassment/coercion
4. Honor killings
5. Female genital mutilation
6. Forced abortions
7. I have not observed

D.9 Child, Disabled, and Elder Abuse

Question: Now thinking about abuses not necessarily tied to Pakistan politics, in the past year, have you observed or do you otherwise have direct knowledge of any of the following incidents of abuse directed against minors, elders, and/or disabled populations in the district in which you live?*

1. Child abandonment
2. Child Psychological Abuse
3. Child neglect/gross negligence
4. Child abuse (physical)
5. Child abuse (sexual)
6. Child labor
7. Child Marriage
8. Disabled population abuse (sexual)
9. Disabled population abuse (physical)
10. Disabled population abuse (financial)
11. Disabled population neglect/gross negligence
12. Elder abandonment
13. Elder neglect/gross negligence
14. Elder abuse (sexual)
15. Elder abuse (physical)
16. Elder abuse (financial)
17. Elder psychological abuse
18. I have not observed any child, disabled or elder abuse in the past two weeks but have in the past month.
19. I have not observed any child, disabled or elder abuse in the past month.

E. Primary Survey Questions and Answer Choices by Category: Pakistan

In the following questions, answers were presented in a randomized order unless otherwise denoted by an asterisk (*). This randomization does not apply to answer options that do not give substantive information (e.g., “Other,” “I don’t know,” “I have not observed [activity] in the past month.”). Additionally, all questions enabled multi-selection unless otherwise indicated by a dagger (†).

E.1 Social Unrest (SU)

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of social demonstration/unrest in the district in which you live?*

1. Peaceful protest or demonstration with police/government intervention
2. Peaceful protest or demonstration without police/government intervention
3. Violent protest or demonstration with police/government intervention
4. Violent protest or demonstration without police/government intervention
5. Riot, mob violence, or other form of civil disorder (e.g. organized looting, property destruction, car burning) with police/government intervention
6. Riot, mob violence, or other form of civil disorder (e.g. organized
7. looting, property destruction, car burning) without police/government intervention
8. Attempted assembly that was violently blocked or otherwise suppressed by
9. governmental actors.
10. Attempted assembly that was violently blocked or otherwise suppressed by
11. non-governmental actors
12. I have not observed any of these activities in the past two weeks but have observed them in the past one month.
13. I have not observed any of these activities in the past month.

E.2 Violence Against Civilians (VAC)

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of violence against any of the following groups in the district in which you live (this violence could be civilian-on-civilian, government-on-civilian, non-state actor-on-civilian, etc.)?

1. Political/Socio-political Groups
2. Religious Minority Groups
3. Muslim Sects
4. Refugee Groups
5. Economic Migrant Groups/Internally Displaced Persons (IDPs)
6. Gender-based Targets (violence against women because they are women, etc)
7. Indigenous Populations
8. Disabled Populations
9. LGBTQI+ Populations
10. Sex workers
11. Ethnic Groups
12. Students
13. Non-affiliated civilians
14. Other
15. I have not observed violence against the groups in the past two weeks but have observed them in the past one month.
16. I have not observed any of the violence in the past month.

Question: What act(s) of violence were committed against [THE GROUP from Q8]?

1. Rape or other form of sexual violence
2. Shooting (e.g. pistol or rifle attack)
3. Bombing (e.g. improvised explosive device attack, land mine)
4. Indirect fire (e.g. rocket attack, mortar attack)
5. Air/Drone Strike
6. Grenade
7. Suicide Bombings
8. Rocket-propelled grenade
9. Rock throwing, stabbing or other non-explosive weapon
10. Punching, kicking, or other form of violence without weapons
11. Abduction/forced disappearance
12. Mob violence
13. Lynching
14. Targeted killing/assassination
15. Other
16. I don’t know

E.3 Violence Against the State (VAS)

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following cases of violent attacks against state forces (e.g. an attack against military, police, personnel, etc.) in the district in which you live?

1. Rape or other form of sexual violence
2. Shooting (e.g. pistol or rifle attack)
3. Improvised Explosive Devices (IEDs) (e.g. vehicle-borne, suicide bombing, rocket-propelled IEDs)
4. Destructive Devices (e.g. Molotov cocktails, fire bombs, other crude improvised bombs)
5. Indirect fire (e.g. rocket attack, mortar attack)
6. Grenade
7. Rocket-propelled grenade
8. Rock throwing, stabbing or other non-explosive weapon
9. Punching, kicking, or other form of violence without weapons
10. Abduction/forced disappearance
11. Mob violence
12. Lynching
13. Other
14. I have not observed violent attacks against state forces in the past two weeks but have in the past month.
15. I have not observed any of the violence against state forces in the past month.

E.4 Labor Rights

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following cases of labor rights abuses (e.g. unpaid labor, hiring discrimination, etc.) in the district in which you live?

1. Unpaid/underpaid Labor
2. Hiring discrimination or wrongful termination based on the ethnic, political, religious, gender or other identity of the applicant.
3. Lack of workers' rights, or infringement on workers' rights based on the ethnic, political, religious, or gender identity of the applicant.
4. Worker intimidation
5. Sexual harassment
6. Workplace discrimination
7. I have not observed any cases of labor rights abuses in the past two weeks but have in the past month.
8. I have not observed any cases of labor rights abuses in the past month.

E.5 Religion

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of violence against members of a religious group attracting broad public attention in the district in which you live?

1. Someone killed for committing Zina (premarital sex)
2. Blasphemy charges being brought against an individual based either on their religious beliefs, or for statements they have made against their own or another religion
3. Damage to or destruction of religious property (e.g. a mosque, church, temple)
4. Damage to or destruction of religious object (e.g. a bible, hijab)
5. Forced conversion
6. Forced marriage
7. Honor killings
8. I have not observed any cases of violence against members of a religious group in the past two weeks but have in the past month.
9. I have not observed any cases of violence against members of a religious group in the past

E.6 Politics

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of violence related to political affiliation, political tensions, or election-related tensions in the district in which you live?

1. Extortion/bribery of government, police, political party officials, etc.
2. Abduction/forced disappearances of government officials, political party members, or civilians for political reasons
3. Torture
4. Extrajudicial killings
5. Violent voter intimidation
6. Being blocked or restricted from voting at or immediately around voting/polling stations
7. Pre-election threats or violence intended to restrict voting
8. Charged or threatened with charges (blasphemy, libel, slander) due to internet activities
9. Internet/telecommunication interruptions/outages due to heightened
10. political activity (elections, protests, etc.)
11. Blockade to restrict traffic or commerce
12. Unjust arrest(s) based on identity or political affiliation
13. Use or threaten to use of libel/slander laws to suppress political discourse
14. Buying community votes: pre- and post-election vote buying
15. Preventing women from voting
16. Threats from militant groups to political parties for participating in the election
17. Leaving politics due to coercion
18. I have not observed any politically motivated violence in the past two weeks but have in the past month.
19. I have not observed any politically motivated violence in the past month.

E.7 Property Crimes

Question: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of property crimes towards someone of a specific religious, ethnic, gender, and/or political identities?

1. Land Seizure- Land Grabbing
2. Destruction or invasion of home/farm
3. Destruction, arbitrary closure or takeover of local business(es)
4. Systematic unlawful evictions
5. Widespread, organized vandalism of property
6. Arson
7. I have not observed any incidents of property crimes in the past two weeks but have in the past month.
8. I have not observed any incidents of property crimes in the past month.

E.8 Gender

Question: In the past year, have you observed or do you otherwise have direct knowledge of any of the following incidences of gender-based violence resulting in public attention in the district in which you live?

1. Forced marriage
2. Forced sterilization
3. Sexual harassment/coercion
4. Honor killings
5. Female genital mutilation
6. Forced abortions
7. No, I have not observed any cases of gender-based violence in the past year

E.9 Child, Disabled, and Elder Abuse

Question: Now thinking about abuses not necessarily tied to Pakistan politics, in the past year, have you observed or do you otherwise have direct knowledge of any of the following incidents of abuse directed against minors, elders, and/or disabled populations in the district in which you live?*

1. Child abandonment
2. Child Psychological Abuse
3. Child neglect/gross negligence
4. Child abuse (physical)
5. Child abuse (sexual)
6. Child labor
7. Child Marriage
8. Disabled population abuse (sexual)
9. Disabled population abuse (physical)
10. Disabled population abuse (financial)
11. Disabled population neglect/gross negligence
12. Elder abandonment
13. Elder neglect/gross negligence
14. Elder abuse (sexual)
15. Elder abuse (physical)
16. Elder abuse (financial)
17. Elder psychological abuse
18. I have not observed any child, disabled or elder abuse in the past two weeks but have in the past month.
19. I have not observed any child, disabled or elder abuse in the past month.

F. Survey Descriptive Statistics

Bangladesh survey waves 1 and 2 ran from December 28, 2023 through May 06, 2024 and August 29 through October 18, 2024, respectively. In Pakistan, the survey ran from January 31 through April 04, 2024. A total of 36,516 individuals answered at least some questions. In Bangladesh, this included 32,283 individuals (wave 1: 16,481, wave 2: 15,800).[49] Of these individuals, 7,669 identified as female (wave 1: 3,108, wave 2: 4,561) and 24,613 identified as males (wave 1: 13,372, wave 2: 11,239), with a mean age across all respondents of ≈34.14 years. The overwhelming majority (25,382) engaged with the survey on smartphones (wave 1: 12,436, wave 2: 12,945,). 6,564 used desktop computers (wave 1: 3,853, wave 2: 2,710). The remaining used smart TVs, tablets, etc. In Pakistan, 4,233 completed some portion of the survey. Of these, 1,110 identified as female and 3,123 identified as males, with a mean age across all respondents of 31.46 years. Most (3,365) engaged with the survey on smartphones. 804 used desktop computers. Those remaining used smart TVs, tablets, etc.

Generally, these statistics compare favorably to existing demographic information on internet users in both countries. For instance, in Bangladesh, 67.66%, 31.81%, and 0.53% of internet users are mobile, desktop, and tablet users, respectively. These percentages largely reflect those observed in the survey: ≈78.62%, ≈20.33%, and ≈0.68%, respectively. For Pakistan, the figures are: 70.43%, 28.77%, and 0.8% vs. ≈79.49%, ≈18.99%, and ≈1.11%, respectively.[50]

Similarly, in Bangladesh, for the adult (18 years of age and older) population ≈19.65% are between 18 and 24 years of age; ≈24.75% are between 25 and 34 years of age; ≈20.52% are between 35 and 44 years of age; ≈15.57% are between 45 and 54 years of age; ≈10.48% are between 55 and 64 years of age; ≈9.02% are between 65 years of age and older.[51] The distribution of reported ages amongst survey respondents is: ≈39.57% are between 18 and 24 years of age; ≈32.42% are between 25 and 34 years of age; ≈11.91% are between 35 and 44 years of age; ≈4.72% are between 45 and 54 years of age; ≈2.87% are between 55 and 64 years of age; <.01.% are between 65 years of age and older.

[49] These numbers sum to 32,281 as two responses were received between the waves.

[50] See: https://gs.statcounter.com/platform-market-share/desktop-mobile-tablet/bangladesh and https://gs.statcounter.com/platform-market-share/desktop-mobile-tablet/pakistan

[51] https://datareportal.com/reports/digital-2023-bangladesh

The patterns in Pakistan are very similar: for the adult (18 years of age and older) population, ≈23.29% are between 18 and 24 years of age; ≈26.62% are between 25 and 34 years of age; ≈19.79% are between 35 and 44 years of age; ≈13.31% are between 45 and 54 years of age; ≈9.28% are between 55 and 64 years of age; ≈7.71% are between 65 years of age and older.[52] The distribution of reported ages amongst survey respondents is: ≈45.55% are between 18 and 24 years of age; ≈28.77% are between 25 and 34 years of age; ≈12.90% are between 35 and 44 years of age; ≈5.27% are between 45 and 54 years of age; ≈2.06% are between 55 and 64 years of age; 0% are between 65 years of age and older.

[52] https://datareportal.com/reports/digital-2025-pakistan

G. Notes About Comparisons With ACLED

First, we adopt and reprint the following remarks made previously by Shaver et al. (2023) regarding the use of existing conflict event data; this project:

"...involves various comparisons with existing conflict event data. We have sought to use these data responsibly and in good faith. The overall goal of this exercise is to identify means by which these existing datasets might be further improved to the collective benefit of the dataset curators and their users, including potential governmental funders. As such, this effort is in no way intended to aid in the development of datasets (or other products) that serve as competitors for these existing conflict event datasets. Instead, the intention is to provide their curators insights about the nature of missing or likely missing incidents from their previous data collection efforts that might inform future collection efforts to their benefit. This research is not intended to negatively depict these conflict datasets or their curators in any manner. Indeed, we have invested a substantial number of work hours in this project precisely because we consider news report-based conflict event datasets to be such a critical resource to academic (and potentially other) communities seeking to understand, forecast, and otherwise engage conceptually with political violence and social unrest globally. To the best of our knowledge, there are presently no viable alternatives to the existing news report-based conflict event datasets that track conflict and/or social unrest on a global basis. As such, and given how extensively these data are used within academia and government/ intergovernmental entities, understanding how these datasets might be further improved is an important public good."

Of the existing conflict event datasets, ACLED is kept up to date in near real time. Furthermore, that data is more highly cited than the others. Thus, it is the media based dataset against which we make our comparisons. ACLED's terms of use are also the most restrictive.

Consistent with ACLED's terms of use, we seek to use their data responsibly and benevolently. The goal of this paper is to give guidelines on how existing datasets may be improved for the benefit of dataset curators and their users, including potential government sponsors. This effort is not intended to aid in the development of datasets (or other products) that serve as competitors for existing conflict event datasets. The intention is to give curators insights about missing or likely missing conflict-event based data on a subnational level which might inform future collection efforts to their benefit. This research is not intended to depict these datasets in a negative light. We have invested a substantial number of hours in this project, and we perceive news-report based conflict event datasets as a critical resource to academic (and potentially other) communities that seek to understand, forecast, and otherwise engage conceptually

political violence, social unrest, and human rights abuses globally. To the best of our knowledge, there are presently no viable alternatives to the existing datasets that track conflict and/or social unrest on a global basis. As such, and given how prevalent this data is in academia and government/intergovernmental entities, understanding how these datasets might be improved is an important public good.

Our methodology for comparing survey results and journalist data to ACLED is identical to the method used previously in Shaver et al. (2023)[53]. We first use location and the date identified to subset the range of events reported by ACLED, then we use event types from the survey respondents and event details from the journalists to match our identified events to those that ACLED reports. We count continuing protests that span across several dates as individual events for each date. This is consistent with ACLED's disaggregation of their events. If an event from ACLED is not found to match our event, we count this event as "newly identified"; whereas if the event was found in ACLED, we count this as "previously identified."

Our survey questions covered the following event and sub-event types from ACLED: protests, peaceful protests, protests with intervention, excessive force against civilians, riots, strategic developments, looting/property destruction, violence against civilians, sexual violence, abduction/forced disappearance, attacks, explosions/remote violence, grenade, drone strike, mob violence, battles, armed clash, non-state actor overtakes territory, and arrests.

From the journalists' data, we chose to exclude events that would not have been tracked by ACLED; these events were extortion, coercion, voter intimidation, and internet blockages or threats of blockages. Evictions and land seizures were included in our comparison data, as these will sometimes appear in strategic development depending on purpose/targets. These events, along with destruction of religious buildings or icons are covered in the looting/property destruction classification. These decisions were made by consulting with the ACLED codebook (ACLED, 2021) and their available primer document on utilizing "strategic development" coded events (ACLED, 2021).

[53] We specify a range of five days for each event to qualify as being included in ACLED. If the journalist's event appears on the same day with matching details (involved actors, location, etc.) then it is assigned a "1". If an ACLED event appears within two days of the journalist even, with matching details- it is assigned a "0.5". If an event falls outside of this five-day range, even if it is suspected of matching, it is recorded as a "0."

Before beginning checks against ACLED data, we manually crosschecked the journalists' tracker (which comprise both survey and non-survey entries) against the survey sheets sent to the journalists. The survey sheets indicated whether a journalist could verify an event, while their tracker indicated whether an entered event was learned about through the survey. We then went through every event in the tracker marked as having come from the survey, matching it to an actual survey sheet event. If any date, location, or other discrepancies arose, we then went back to the journalists to clarify.
This process was true for the two journalists in Pakistan and the second journalist in Bangladesh, but there is some uncertainty with this crosschecking processing for the first journalist in Bangladesh. Given extenuating personal circumstances, we only had access to the first journalist's tracker- we were unable to check those against the survey sheet. Given our conservativeness with ACLED checks (a five-day window for a partially matching event), this does not suggest any reason to think the data is of lesser quality than those with completed checks.

G.1 Matching Event Types from ACLED to Survey Data

First, in order to compare like events, the event types reported through the surveys were matched with event types tracked by ACLED. The majority of the event types matched seamlessly; though, some types were combined to ensure comparability. Table 1 displays those comparisons.

With respect to incidents of politically motivated property destruction, the relevant survey response referred specifically to *Widespread, organized vandalism of property*. Accordingly, we include affirmative responses to this question in our comparisons with ACLED's property destruction as we sought through the question text to rule out smaller scale vandalism that may fall outside of ACLED's collection efforts. Regardless, however, if reported cases of widespread, organized vandalism of property are excluded from our analyses, results are substantively unchanged.

Finally, in our comparisons with cases of property destruction tracked by ACLED, we include instances of damage to property. We do so as, although the category name "property destruction" might suggest that only cases of wholescale destruction are included, we can see from the cases that ACLED tracks that it also includes instances of damage, as the following event from that dataset shows: "Property destruction: On 24 January 2025, assailants (likely Muslims based on the issue) vandalized the shrine of Shah Sufi Shah Fakir in Gangutia union, Dhamrai upazila (Dhaka), over claimed un-Islamic activities taking place at Sufi shrines across the country.").

Table 1. Mapping between ACLED categories and survey event types

Category	ACLED			Survey Data: Event Type
	Disorder Type	**Event Type**	**Sub-event Type**	
Social Unrest	Demonstrations	Protests	Peaceful Protests	Peaceful protest without police
	Demonstrations	Protests	Protest with Intervention	Peaceful protest with police
	Political violence	Riots	Mob violence	Violent protest with police Violent protest without police Riot/Mob violence with police Riot/Mob violence without police
	Demonstrations	Riots	Violent demonstration	
	Demonstrations	Protests	Excessive force against protesters	
Violence Against Civilians	Political violence	Violence against civilians		Violence against civilians
Destruction of Property	Strategic developments	Strategic developments	Looting/Property destruction	Destruction of home Destruction of local business Arson Damage to religious property Widespread vandalism

H. Coding Methodology Bangladesh

The U.S. Department of State (2023) provided high-level categorizations of human rights abuses. Within each category, specific abuses/issues were identified from various human rights resources—e.g. on violations of religious freedom and/or censorship (Global Rights Project, 2023; U.S. Commission on International Religious Freedom, 2023; Gargi Das Chomok, 2033), refugee issues (U.S. Commission on International Religious Freedom, 2022; United States Com- mission on International Religious Freedom, 2023), discrimination against women (Human Rights Watch, 2020; Bardall et al., 2020), labor abuses (Bureau of Democracy, Human Rights, and Labor; Department of Labor, 2022; International Trade Union Confederation, 2021), politics (Human Rights Watch, 2023a; Hafner-Burton et al., 2018; Alam and Pathi, 2023), property abuses (Times of India, 2021; Internal Displacement Monitoring Centre, 2015), and child/elder abuse (World Health Organization, 2022; Department of Labor, 2022), etc. Lastly, some events, though intriguing, were ultimately excluded based on the journalists' capability to verify such event types.

Most of the survey questions include two non-observation questions: "I have not observed any cases of [event type] in the past two weeks but have in the past month" and "I have not observed any cases of [event type] in the past month." This was multifunctional—helped identify less frequent event types and allowed the survey flow to direct respondents to alternative questions, thus utilizing the survey to the fullest extent.

The labor rights questions were created using common knowledge, labor and economic sources, and a list of main industries, provided by the journalists in each country.

The questions were structured and phrased to be verifiable within the larger administrative districts (Humanitarian Data Exchange, 2020, 2022), which also allowed for easier verification in smaller districts.

For the gender-based violence category, there were many overlaps with other event categories. Additionally, many of the questions we were interested in would be both difficult to verify and sensitive. As such, event detail questions were removed and respondents were shown these questions only if they had no observation of any other events. The same methodology was used for the child, disabled, and elder abuse category.

In order to ensure both journalist verifiability and comparability with ACLED, questions were phrased to be capture events that are systematic and widespread in nature, i.e. “Destruction, arbitrary closure or takeover of local business(es),” “systematic unlawful evictions,” and “widespread, organized vandalism of property.”

Initially, the list of religious groups included as perpetrators/victims was informed by various sources (Akbaba et al, 2011; U.S. Department of State, 2022), subsequently informed by the contracted journalists (e.g., combining groups, removing groups). The finalized list of included religious groups for Bangladesh can be found in the table below.

Table 2. Bangladesh Religious Groups

Muslim (Ahmadi)	Muslim (Sunni)	Muslim (Shiite)	Muslim (Other)	Rohingya
Hindu (Brahmin)	Hindu (Baidya)	Hindu (Kashtriya)	Hindu (Dalits)	Hindu (Vaishya)
Hindu (other)	Christian	Sikh	Buddhist	Bahá´í
Kalash	Parsi	Zikri	Animists	ISKCON member
Agnostics	Atheists	Other	I don’t know	

The list of regions in Bangladesh was created using (Humanitarian Data Exchange, 2020) which dictated the ADM1, ADM2 and ADM3 sections in the region.

The social unrest event types were informed by observed ACLED data from 2023 (ACLED, 2021) and work that the journalists in the region conducted previously. Consulting both of these sources, we identified groups that could have perpetrated such events.

The political groups included in the survey were based on information provided by from the journalists and from political groups mentioned in Rahman (2022). The comprehensive list can be found in Table 3.

Table 3. Bangladesh Political Groups

AB: Amar Bangladesh Party	ARSA: Arakan Rohingya Salvation Army	ASL: Bangladesh Awami Swechchasebak League
Awami League	BJSD: Bangladesh Jatiyatabadi Srimak Dal	BNP: Bangladesh Nationalist Party
BSRC: Bangladesh Sadharon Chhatra Odhikar Songrokkhon Parishad	Chhatra League	DYF: Democratic Youth Forum
Farmers Labor Group	GA: Ganosamhati Andolon	GAP: Gana Adhikar Party
GM: Ganatantra Mancha	ICS: Islami Chhatra Shibir	Jamaat-e-Islami Bangladesh
Jatiya Party	JCD: Bangladesh Jatiotabadi Chatra Dal	JKD: Jatiyabadi Krishak Dal
JMC: National Freedom Fighter Council	Jubo Dal	Jubo Mohila League
LDA: Left Democratic Alliance	PCP: Parbatya Chattagram Pahari	RSO: Rohingya Solidarity Organization
SFB: Students' Federation of Bangladesh	SSF: Socialist Students' Front	Refugees (Myanmar)
Other	I don't know	

When including possible targets of violence against, we used broad categories (e.g., religious groups, political groups) informed by the journalists and common knowledge. For perpetrators of violence against civilians, we removed groups such as disabled populations and LGBTQ+ groups. This was in recognition of populations that historically are not perpetrators of significant violence.

When compiling our list of specific acts of violence (shootings, bombings, stabbings, etc.) we used a combination of information from our contracted independent journalist in the region, and from the ACLED dataset (ACLED, 2021). We pulled categories from the ACLED dataset so as to make it easier and more accurate to compare the results from our survey to ACLED's data.

For groups that commit violence against the state, we again consulted observed data from ACLED (ACLED, 2021) and sources such as the Counter Terrorism Guide (2022). Targets of violence against the state were limited to police, military, and government officials. We did not include government buildings, as this is a category that is included under property destruction.

I. Coding Methodology Pakistan

For the Pakistan version of the survey, we used the existing framework of the survey in Bangladesh and adjusted several aspects—such as the political and religious group identifiers, to be an accurate depiction of the country's demographics and electoral system. The classification and title of different geographic levels were also changed. In Bangladesh, the highest geographic level was classified as division, the following level as district, and the lowest level as subdistrict/upazila. Conversely, in Pakistan these categories were identified as territory/province, district, and tehsil, respectively. Pakistani religious groups were identified using various sources, including Hasnain (2021); State Department Office of International Religious Freedom (2022); Qadeer (2006); ETH Zurich (2021); UNHCR (2023); Bormann (2017), as well as input from the Pakistani journalists—who recommended the removal of redundant identifiers and the addition of two denominations. The final list of Pakistani religious groups is shown in Table 4.

Table 4. Pakistani Religious Groups

Ahmadis	Christian	Hindu (Dalits)	Hindu (Other)	Sikh
Rohingya	Muslim (Shiite)	Muslim (Sunni)	Muslim (Deobandi)	Muslim (Barelvi)
Muslim (other)	Kalash	Baha'i	Parsis (Zoroastrians)	Jain
Buddhist	Kihals	ISKCON member	Zikri	Agnostics
Atheists	Other	I don't know		

The list of political groups/insurgent groups in Pakistan was informed by limited sources, namely Election Commission of Pakistan (2024) and Chughtai and Hashim (2018)—each of which had extensive identifications. The final lists, which included journalist-identified options, are found in Tables 5 and 6.

Table 5. Pakistani Political Parties

Awami National Party	**Balochistan Awami Party**	**BNP: Balochistan National Party**
BRAS: Baloch Raaji Ajoi Sangar	BRG: Baloch Republican Guard	BSO: Baloch Students Organization
BYC: Baloch Yakjehti Committee	Give Gwadar Rights	Kech Civil Society
Muttahida Qaumi Movement	Pakistan Muslim League-Nawaz	Pakistan People's Party
Pakistan Tehreek-e-Insaf	Pashtun Tahafuz Movement	Refugees
Refugees (Afghanistan)	Other	I don't know

Table 6. Pakistani Insurgent Groups

BLA: Baloch Liberation Army	**BLF: Baloch Liberation Front**	**ISKP: Islamic State of Khorasan Province**
ISPP: Islamic State of Pakistan Province	Jaish-e-Muhammad	Lashkar-e-Jhangvi
Sindhudesh Liberation Army	Tehreek-e-Taliban Pakistan	The Ghazyano Caravan
UBA: United Baloch Army	Other	I don't know

J. Changes to Attack Type

We found that attack types such as 1. air/drone strike, and 2. suicide bombings had to be added to the Pakistan survey as new options, based on feedback from our independent journalists in the region.

The initial survey responses in Bangladesh had a high concentration of “Other” responses in the Violence against the State category. To combat this, we added “Improvised Explosive Devices (IEDs) (e.g. vehicle-borne, suicide bombing, rocket-propelled IEDs)” and “Destructive Devices (e.g. Molotov cocktails, fire bombs, other crude improvised bombs)” as response options due to their prominence in Pakistan (Halder, 2024; Human Rights Watch, 2023b; Committee on Foreign Relations, 2012).

J.1 Changes to Industry

Due to a difference in the dominant industries between Bangladesh and Pakistan, we edited our list of industries. We added 1. port, 2. automotive, 3. chemical, 4. mining, 5. services, and 6. informal trade.

K. Survey Questions And Specific Associated Follow-On Questions

This section displays all of the survey questions used in our Bangladesh survey. For differences between that and the Pakistan survey, please reference appendix section D.9. In the following questions, answers were presented in a randomized order unless otherwise denoted by an asterisk (*). This randomization does not apply to answer options that do not give substantive information (e.g., “Other,” “I don’t know,” ”I have not observed [activity] in the past month.”). Additionally, all questions enabled multi-selection unless otherwise indicated by a dagger (†).

K.1 Identifiers

- Q1: Which division do you live in?* †
- Q2: Which district do you live in?* †

K.2 Preamble

- Q3: We are going to ask if over the past two weeks, you have observed or do you otherwise have direct knowledge of a series of different events. We are especially interested in, but not exclusively, incidents about which you may be aware that have not received significant news media attention. We are interested in incidents of discrimination, persecution, violence, etc. against individuals that are based broadly on Bangladeshi politics. This includes violence against individuals on the basis of real or perceived characteristics of those individuals—e.g. if an individual or set of individuals are targeted on the basis of their race; religion; ethnicity; nationality; status as a refugee, asylum seeker or forcibly displaced person; age; gender; sexual orientation (LGBTQI+); for having mental or physical disabilities; etc.

K.3 Social Unrest (SU)

- Q4: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of social demonstration/unrest in the district in which you live?*
- Q5: Which group(s) initiated the [event FROM Q4]?
- Q5a: Which specific religious group(s) initiated the [event FROM Q4]?*
- Q5b: Which specific political/sociopolitical group(s) initiated the [event FROM Q4]?*
- Q6: In what sub-district/upazila did the [event FROM Q4] take place?*†
- Q7: Please provide your best estimate of the date on which the [event FROM Q4] took place. You can enter a date directly in the space below in the mm/dd/yyyy format (e.g. 12/30/2023).*†

K.4 Violence Against Civilians (VAC)

- Q8: In the past two weeks, have you observed or do you otherwise have direct knowledge of violence against any of the following groups in the district in which you live (this violence could be civilian-on-civilian, government-on-civilian, non-state actor-on-civilian, etc.)?
- Q9: What act(s) of violence were committed against [EVENT from Q8]?
- Q10: Which group(s) committed the [EVENT from Q9]?
- Q10a: Which specific political/socio-political group(s) initiated the [EVENT from Q9]?*
- Q10b: Which specific religious group(s) initiated the [EVENT from Q9]?*
- Q11: In what sub-district/upazila did the [EVENT from Q9] take place?*†
- Q12: Please provide your best estimate of the date on which the [EVENT from Q9] took place. You can enter a date directly in the space below in the mm/dd/yyyy format (e.g. 12/30/2023).*†

K.5 Violence Against the State (VAS)

- Q13: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following cases of violent attacks against state forces (e.g. an attack against military, police, personnel, etc.) in the district in which you live?
- Q14: Which group(s) were responsible for [EVENT from Q13]?
- Q14a: Which specific religious group(s) initiated [EVENT from Q13]?*
- Q14b: Which specific political/sociopolitical group(s) initiated [EVENT from Q13]?*
- Q15: Which group was targeted by [EVENT from Q13]?†
- Q16: In what sub-district/upazila did the [EVENT from Q13]take place?*†
- Q17: Please provide your best estimate of the date on which the [EVENT from Q13] took place. You can enter a date directly in the space below in the mm/dd/yyyy format (e.g. 12/30/2023).*†

K.6 Labor Rights

- Q18: In the past two-weeks, have you observed or do you otherwise have direct knowledge of any of the following cases of labor rights abuses (e.g. unpaid labor, hiring discrimination, etc.) in the district in which you live?
- Q19: Against which group(s) were [EVENT from Q18] committed?
- Q20: In which industry did [EVENT from Q18] take place?†

K.7 Religion

- Q21. In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of violence against members of a religious group attracting broad public attention in the district in which you live?
- Q22. What religion did the victim(s) of [EVENT from Q21] belong to?*
- Q23. Which group(s) committed [EVENT from Q21]?*
- Q24. In what sub-district/upazila did the [EVENT from Q21] occur?*†
- Q25. Please provide your best estimate of the date on which the [EVENT from Q21] took place. You can enter a date directly in the space below in the mm/dd/yyyy format (e.g. 12/30/2023).*†

K.8 Politics

- Q26: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of violence related to political affiliation, political tensions, or election-related tensions in the district in which you live?
- Q27: What group(s) were the target of [EVENT from Q26]?*
- Q28: Which group(s) committed [EVENT from Q26]?*
- Q29: In what sub-district/upazila did the [EVENT from Q26] occur?*†
- Q30: Please provide your best estimate of the date on which the [EVENT from Q26] took place. You can enter a date directly in the space below in the mm/dd/yyyy format (e.g. 12/30/2023).*†

K.9 Property Crimes

- Q31: In the past two weeks, have you observed or do you otherwise have direct knowledge of any of the following incidents of property crimes towards someone of a specific religious, ethnic, gender, and/or political identities?
- Q32: What identity group does the victim(s) of [EVENT from Q31] belong
- to?†
- Q33: In what sub-district/upazila did the [EVENT from Q31] occur?*†
- Q34: Please provide your best estimate of the date on which the [EVENT from Q31] took place. You can enter a date directly in the space below in the mm/dd/yyyy format (e.g. 12/30/2023).*†

K.10 Gender (GBV)

- Q35: In the past year, have you observed or do you otherwise have direct knowledge of any of the following incidences of gender-based violence resulting in public attention in the district in which you live?

K.11 Child, Disabled, and Elder Abuse

- Q36: Now thinking about abuses not necessarily tied to Bangladeshi politics, in the past year, have you observed or do you otherwise have direct knowledge of any of the following incidents of abuse directed against minors, elders, and/or disabled populations in the district in which you live?*

L. Survey Randomization Logic/Survey Limitations

All primary questions the respondents see for the event types are randomized, with the exception of Q4. When asking about social unrest, we listed identical event types attempting to understand police/state force involvement. Given the similarity of the event type (e.g., “Peaceful protest with police/government intervention” and “Peaceful protest without police/government intervention”), we grouped these answers together for clarity. Additionally, answer choices for specific group names were listed in alphabetical/categorical order (e.g., different sects of a religious group, political parties). There are several limitations to the way the survey was formed. First, there was a limitation in skipping questions. There was no option to skip questions in this survey; the only option was to quit the survey entirely. Another limitation was with our final two questions, the Gender and Child, Disabled and Elder Abuse sections. These are only shown after a respondent declared “I have not seen any [blank] in the past month.” Finally, there was a limitation in the manual date-entry solution to our search for a date. This option was demonstrated to not be amicable to our survey respondents, severely limiting the number of completes in the survey. We addressed this by instead adding upper- and lower-bound date selections: “1–2 days ago,” “3–4 days ago,” “5–6 days ago,” “7–8 days ago,” “9–10 days ago,” “11–12 days ago,” and “13–14 days ago.”

M. Ethical Considerations

Research involving anonymous, online surveys and concurrent engagement with journalists received approval from the University of California-Merced Institutional Review Board #UCM2023- 206.

M.1 Risks

Schock et al. (2015) describes how discussing cases of human suffering and violence that the respondent may have witnessed could produce some degree of emotional distress; "Previous studies on the specific nature of intrusions provide a possible explanation, because they suggested that intrusive memories are stimuli that acquire the status of warning signals through temporal association with the trauma—that is, stimuli that, if encountered again, would indicate impending danger." However, pressure to discuss/reflect on such topics were minimized as the survey was fully optional and may simply not be taken if the respondent did not wish to participate. We offered no monetary incentives to respondents in order to ensure that there was no bias to participate, leaving respondents to participate only because the topic held salience for them or they simply wanted to take a survey. In addition, the informed consent document reiterated that most of the questions concerned violence, social unrest, and/or human rights violations and may be difficult or upsetting, and emphasized that respondents should have only proceeded if they were comfortable discussing these topics. Once the respondents felt distressed or upset during the survey, they could quit the survey immediately.

M.1.1 Risk Mitigation

Some of the questions we ask relate to cases of human suffering and violence that the respondent may have witnessed. Discussing such topics could produce some degree of emotional distress.

However, pressure to discuss/reflect on such topics was minimized as the survey was fully optional and could simply not be taken if the respondent did not wish to participate. To ensure that there is no bias to participate, "No incentives for responding [were] offered - which eliminates the possibility of incentive bias - and participants [were] able to exit the survey at any time. Because respondents [were] not given incentives to participate, they [answered] because the topic [held] salience for them or because surveys or message tests are somewhat of a novelty" (RIWI 2023). In addition, the informed consent document reiterated that most of the questions concern violence, social unrest, and/or human rights violations and may have been difficult or upsetting, and emphasized that respondents should only proceed if they were comfortable discussing these topics.

Once the respondents felt distressed or upset during the survey, they could quit the survey immediately, there is not restriction that a respondent has to complete the whole survey.

The surveys were not conducted with minors, as RIWI states that their "website is not designed to attract children and it is not our intent to collect personal data from children under the age of consent in their country of residence. We, therefore, request that children under the age of consent do not submit any personal data to us" (RIWI, 2023b). To guard against minors taking the survey, the survey began with "pickers" which required respondents to enter their age and place where they reside. If they entered an age below 18, or if they resided in a region that was not Pakistan or Bangladesh, the survey would close for them automatically before they get access to actual survey questions. Once removed, it was impossible to access the survey again, regardless of whether the applicant attempts to reload the page.

Regarding respondent privacy, breaches of security concerning privacy were not a concern as RIWI collects no personally identifiable information, and any trace of the survey would disappear from the respondent's computer upon completion of the survey (RIWI, 2023a). RIWI only records respondent-provided (but not personally identifiable) demographics information such as respondent location (tehsils or sub-divisions, similar to counties), age (to ensure that respondents are adults), and ethnic-religious identity. We asked respondents to tell us their ethnic-religious identity to give us deeper insights into the type of violence, social unrest, and/or human rights violations that certain minority groups witness or undergo, in comparison to other populations. Thus, we expected the risk of a breach of the respondent's personal information to be very minimal. In not compensating respondents, we minimized the risk of them being identified by the digital footprint of a payment. In addition, RIWI keeps track of which countries they consider "sensitive" in terms of internet censorship. For "sensitive" countries such as China, which they have observed shutting down points of entry to the survey, RIWI takes extra security measures.

Participants were only surveyed using RIWI technology which provides several provisions and precautions to minimize the risk of a breach of respondent privacy. Respondents retained their anonymity. Before beginning the survey, respondents were advised of their privacy and security and were recommended not to take the survey in a public area. Survey respondents are anonymous and thus protected through RIWI's data collection policy; "RIWI recruits only random and anonymous respondents who do not provide personally identifiable information. RIWI meets all US, CA and EU privacy rules, including GDPR" (RIWI, 2023b). After a respondent leaves the survey, there is no evidence on their device that they participated in the survey.

In addition, “RIWI surveys cannot be monitored, filtered, or blocked by state surveillance or Internet control, and are not susceptible to the increasing prevalence of ad block technologies. Avoiding the collection of personally identifiable information from respondents is critical for data collection on sensitive issues and in regions where social pressures exert outsized influence on public opinion” (RIWI, 2023b).

M.1.2 Journalist Risks and Mitigation

As respondents completed the surveys, we sent the relevant details to the journalists for their follow-on verification work. This was done to ensure that journalists could follow up on the issues in a timely manner, increasing the likelihood that true events could be verified.

The survey data itself was NOT shared directly with the journalists. Instead, members of the research team shared this information only after reviewing it themselves. This ensured that no sensitive details were shared with the journalists that should not have been. Given the current survey design (typically selecting amongst displayed items), such transmission of sensitive data should not have been possible regardless. Respondents would have open-entry type questions only for dates. So, it was generally not be possible for them to share sensitive details. This firewall between the raw data and journalists helped prevent any such transmission, however unlikely.

For each relevant incident, the journalists sought to confirm whether it occurred and, if so, to acquire additional details about the incident. (Given survey space constraints, we only asked survey respondents about basic details of each incident – when, where, general parties involved, etc. Thus, our contractual agreements with the journalists included obtaining additional de- tails about the verified incidents – e.g. for a protest, what number, if any, of participants were injured?) To do so, the journalists engaged in whatever investigative steps they deemed appropriate, consistent with their history of working on such issues as professional journalists.

There is also a risk posed to our journalists and to their network as they go about verifying RIWI survey data. We have no way of completely ensuring journalist safety; however, these journalists operate as independent contractors in a role that they have already professionally assumed and for which they therefore have already accepted the inherent risks of. We will also be maintaining our journalists’ anonymity in all reports.

M.2 Informed Consent

Potential participants were shown the following informed consent language:

"University of California, Merced researchers seek to identify incidents in Bangladesh that receive limited news media attention. To do so, they are conducting this anonymous, online survey in which you would be asked a small number of questions, which should take approximately 10 minutes to complete. We understand that discussing events you have witnessed might result in some level of emotional distress. Therefore, your participation is voluntary. You are free to answer or skip any questions or leave at any time, particularly if you are uncomfortable with any question asked. You will not be asked for any personally identifiable information like name, address, or date of birth. There is no cost to you to complete the survey and you will not be compensated. There will be no benefit to you for participating. UC Merced's Institutional Review Board has approved this survey. Should you have any questions or concerns about this study, please contact our research team at ashaver@ucmerced.edu or UC Merced's Office of Research at irbchair@ucmerced.edu. Continuing with the survey indicates your willingness to participate in this study and confirms that you are at least 18 years old and presently reside in [Bangladesh or Pakistan]. To learn more, click on the 'Privacy Policy' link below."

Given that the recollection of traumatic events may adversely affect participants' mental health, we incorporated a relevant resource at the end of the survey:

> *"Thank you for your participation! In case you need help or would like to talk to someone about the incidents reported by you, please contact the following resources: Kane Pete Roi. https://www.facebook.com/profile.php/?id=100069104871429&name=xhpntfblite profiletabbar&profiletabitemselected=about; Telephone: 09612-119911."*

N. Survey Interruptions & Errata

N.1 Bangladesh

The survey ran completely from December 28, 2023 to May 6, 2024, which we refer to as "Wave 1." There was a brief pause due to a technical error from January 8-9, 2024. With all of the survey responses received, we sent the finalized data to our contracted journalist in Bangladesh. The original quota was for 600 complete surveys was later extended to 915. Unfortunately, while our journalist was engaged in the verification work, they experienced a personal emergency that disrupted the work. Ultimately, they were able to provide the requested set of incidents they tracked but not the verifications. Accordingly, a new journalist was contracted, the contract with RIWI was extended, and the survey was run again (Wave 2) between August 29, 2024, and October 18, 2024. The new journalist was contracted to verify 700 additional complete interviews from the second wave of the survey.

We also note several journalist errors in input. There were several date entries for which the month changes to January in a way that suggests the actual date of an event happened in February. For two dates in the Social Unrest category, the journalist listed 2/30/2024 as the date of the event. As such a day does not exist, both were changed to 2/29/2024. In two cases, end dates were listed before start dates. These were switched. In another case, a Human Rights case was listed on 30/01/23, and was changed to 30/01/24 to fit standard American formatting. In the event of any changes, journalists were contacted to ensure accuracy.

N.2 Pakistan

The survey was conducted over a shorter period of time than originally contracted—running from January 31, 2024 to April 4, 2024. This decision was made in order to allow the contracted journalists to focus on the large numbers of survey responses already received. The original contracted number of complete survey responses was 600, but was revised to 285 complete responses in light of the above. After the survey was completed, we revised the terms of the contract with RIWI so that the number of completes allocated between the two countries were reapportioned between the two, resulting in the aforementioned extension to the number of complete responses in Bangladesh to 915.

While sharing the selected survey responses with the journalist, there were duplicate "city" and "region" columns with differing information. One pair indicated the location in which the survey was taken, the other indicating the respondents' self-reported locations. The columns indicating where the survey was completed was removed from all shared data, starting on March 21, 2024.

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